# NATURAL SCIENCE:

## A Monthly Review of Scientific Progress.

No. 20. Vol III. OCTOBER, 1893.

## NOTES AND COMMENTS.

### The Organisation of Science.

ONE of the most gratifying announcements of the month in British Science, is a statement by Lord Rayleigh to the effect that the Royal Society of London has lately appointed a committee to consider, in an entirely impartial manner, whether or not the procedure of that body can be altered with advantage. His lordship's speech was made before Section A of the British Association, in the course of a discussion on the publication of scientific papers. The physicists have been much excited of late on the publication question, and it was thought that a debate at Nottingham might do something further towards a solution of difficulties.

The anonymous author of a little pamphlet "On the Organisation of Science," which we reviewed in June, 1892, seems to have been the first, in recent years, to issue a reasoned plea for a more systematic method of publication of scientific papers, and the centralisation of the publishing authority; but as we pointed out at the time, and as Lord Rayleigh remarked at Nottingham, there is the great difficulty of censorship and the fear of a dominating clique. One of the essentials for progress is absolute freedom; and so long as there are competitive journals and rival societies, there is little danger of any novel views being suppressed merely on account of their unorthodoxy. The physicists and mathematicians of the British Association seem to have been agreed that there is no real necessity for interference with the "disorganisation" of present arrangements; but that there ought to be some recognised central authority for the periodical issue of volumes of abstracts relating to each branch of Science. This is the work that Professor Rücker and some others would like to see undertaken, or at any rate supervised, by the Royal Society; and the committee will certainly confer a great boon upon

workers if they are able to make any suggestions for rendering records of progress more systematic and elaborate than they are at present.

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## Scientific Linguistics.

When a layman asks a naturalist why he invents and employs such a multitude of incomprehensible technical terms, the common reply is that exact ideas necessitate a precise and universally-understood nomenclature. We wonder how this explanation would apply to the terms of "Auxology" or "Bioplastology" just discussed again by Professor A. Hyatt in the *Zoologischer Anzeiger* (concluded August 28). We should like to know how much scientific precision there is in the determination of the nepionic, metaneonic, gerontic, parageronic, etc., stages of any organism; and what grain of solid fact, as compared with mere speculation, in the so-called definition of the phylonepionic, phyloneanic, phylogerontic, etc., phases of development in any group of animals. We may be enslaved by some prejudice, and our patience may have been ruffled in the attempt to decipher some recent writings of American authors on fossil shells; but we cannot help uttering a protest against the clothing of a tissue of hypothetical fabrications in the garb of a precisely-defined scientific nomenclature. It is, of course, a matter of everyday knowledge that each organism passes through several marked stages in its individual development; and most naturalists will admit that a good deal of evidence is constantly being discovered as to the evolution of races, genera, and species in course of time. Nevertheless, there is as yet nothing very exact in this knowledge; and until that exactitude is reached, the invention and application of scientific terms is a delusion and a snare, and a veritable hindrance to progress.

The embryologists need not be considered in the matter; for they do not seem to have taken kindly to the classification of stages of development by those whose practical acquaintance with the study of embryos is, for the most part, probably *nil*. The latest work on the subject (Marshall's "Vertebrate Embryology") does not even mention that any such classification has been attempted. When they begin to discuss protembryos, mesembryos, metembryos, etc., it will be time to return to the subject, and examine the cogency of their reasoning.

It is with the speculative palæontologists that we have to deal. It is they who arrange ammonites and brachiopods in rows, and unfold to us, with surprising confidence, the history of a genus, a species, or a race. The manner in which they are studying growth, variation, and the stratigraphical sequence of forms, by the examination of enormous collections, is one of the most gratifying signs of the times. This is certainly the only method by which the modern ideas of Biology can be advanced. It ought, however, to suffice at present to record clearly and simply the facts, carefully distinguishing all legitimate suggestions as to their likely explanation; and, to our mind,

it is one of the greatest misfortunes to entomb and obscure these plain facts in an array of would-be scientific expressions, which are nothing more than the outcome of the guesswork of each individual author.

If there is any logic in this attempt at a nomenclature, why do not Messrs. Hyatt, Beecher, Buckman, Bather, and its other advocates complete its definition in general terms? Why do they not show how to correlate the growth-stages, for example, of a brachiopod with those of a mollusc, and these, again, with those of a mammal? How do they determine which period in the life of a snail is scientifically equivalent to one of a crab? If no reasoned answer to these questions can be given, then, assuredly, the time has not yet arrived for applying technical terms to the various periods of the life-cycle.

In certain quarters, indeed, Biology seems to have reached a phase equivalent to that in which the originators of the Geological Society of London found the "Theory of the Earth" in 1807. That Society was established to combat the spirit of the day, "to multiply and record observations, and patiently to await the result"; and it was at first their favourite maxim "that the time was not yet come for a general system of geology, but that all must be content for many years to be exclusively engaged in furnishing materials for future generalisations." The older Societies dealing with Biology in Europe are wise enough still to withhold their patronage from any such reckless developments of "Auxology" and "Bioplastology" as we find in some magazines and transactions elsewhere; and the sooner the false appearance of this "whited sepulchre"—this clothing of pure hypothesis in a garment of precise terminology—is demolished, the more conducive will it be to sound progress.

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## Lake Superior.

An important geological memoir bearing upon the question of the local instability of the earth's crust has been issued by the Survey of Minnesota, U.S.A.[1] The evidence of changes of level on oceanic coast-lines is, of course, easily enough observed; but nearly everywhere inland very little progress has hitherto been made in detecting such movements. When, however, the area is large enough, recent unequal changes of level can usually be discovered by the study of the topography, such as the nature of stream-erosion, the distribution of sediment, and the deformation of the abandoned shore-lines of lakes. It is from this point of view that Professor Lawson has studied the old beaches to the north of Lake Superior.

It is now generally recognised that streams are very sensitive to any change in the slope of their trenches or of any portion of them.

[1] A. C. Lawson, "Sketch of the Coastal Topography of the north side of Lake Superior, with special reference to the Abandoned Strands of Lake Warren (the greatest of the late Quaternary Lakes of North America)," *Geol. & Nat. Hist. Surv. Minnesota, 20th Annual Report*, year 1891 (1893), pp. 181–289, with illustrations.

There is a uniform minimum slope which they constantly seek to establish and maintain. Any movement of the land leaves its record in a change in the intensity of the action of the stream, whether it be cutting or depositing; and in non-glaciated regions streams are now systematically examined by geologists for the purpose of ascertaining whether the country traversed by them has been uplifted or depressed, or has maintained a fairly constant altitude. Evidence of this kind, however, is not always available, and it is necessary to turn to other features. For instance, there are some regions in which the shores of ancient lakes can be traced for many miles—in the centre of North America sometimes for hundreds of miles. These old beaches must, of course, have been originally horizontal, and if, therefore, any one of them appears now to be displaced, variously inclined, and at somewhat different levels in different parts, there is clear evidence of movements of the crust since the lake dwindled or disappeared. Such evidence of earth-movements has been known for some years in North America on either side of the Lake Superior basin,[1] and Professor A. C. Lawson's investigation of the old beaches to the north of the present Lake Superior was undertaken with the object of ascertaining how far the effect of movements could be traced in that region. A beautifully illustrated detailed report leads to the conclusion that there once existed an enormous "Lake Warren" (to use Professor Spencer's term) probably at least twice as great as the combined areas of the present lakes Superior, Michigan, and Huron, or about 150,000 square miles. In some places as many as nineteen distinct beach-lines can be traced along the northern shore of Lake Superior, and Professor Lawson believes there is evidence of at least thirty-two definite stages in the recession of the lake. It seems probable that the lowering of the water was gradual, though perhaps varying in amount at different times; and it is hardly possible to believe that the existence of ice barriers had any connection with the phenomena. It is likely, therefore, that the local warping of the earth's crust, in some region far removed from Lake Superior, is accountable for the lowering of the land barrier which held back the waters of "Lake Warren." The outlet may even have been at different places at different times in consequence of the unequal continental warping of which there is evidence elsewhere. Professor Lawson enumerates the possible situation of some of these outlets, and makes many suggestions that will be of much service to future observers, both in this and other regions.

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### The Feeding of Fishes.

It is well-known that the sense of smell in fishes is very keen, and that all use it more or less in feeding, whether or not sight aids

[1] *See* "Great Lakes," by Clement Reid, Nat. Sci., vol. i., p. 117.

them in the process. Some further experiments on the subject have been made by Mr. Gregg Wilson in the Plymouth Marine Biological Association, and the following observations from his recent report to the British Association will be read with especial interest :—

"So far as I could determine, fish that are not *very hungry* habitually smell food before taking it. The pollack seems usually to be ready for a meal, and on almost all occasions when anything eatable is thrown into the tank in which it is swimming, it rushes towards it and bolts it. It does not hesitate to take stale food or food that has been steeped in strong smelling fluids; and time after time I have been amused to see its too-late repentance, after it had swallowed clams that had been saturated with alcohol, chloroform, turpentine, etc. It is only when it is satiated with fresh food or disgusted with what is nauseous that it takes the precaution to smell before eating. On the other hand, various fish that are equally keen-sighted, and habitually recognise their food by the use of their eyes, are more prudent. The whiting (*Gadus merlangus*), for instance, appears to pay much more attention to smell, and, as a rule, turns about and withdraws on approaching within a few inches of high-smelling objects that the pollack would take without hesitation. Even whiting, however, cease to be delicate if they are very hungry, and if other fish are present to compete for the food that is thrown to them. In such circumstances bait that is very distasteful may be taken by even the most cautious of sight-feeders; and likewise, in such circumstances, a quite smell-less artificial bait may be successfully employed. Where large shoals of fish are, there are likely to be many that are very hungry, and the consequent keen competition will lead to hasty feeding by sight alone; and hence it is, probably, that lead-baits are successfully employed in cod fishing in the Moray Firth and off the Northern Islands, while they are of no avail among the scanty fish further south.

"It may be said that in these cases the fish actually *search* for their food by sight alone, and merely test the quality of what they have found by smelling it; and Bateson quite recognised this. But more is possible: *habitual sight-feeders can be induced to hunt by smell alone*. The pollack, which is such a pronounced sight-feeder that it will take a hook baited with a white feather or a little bit of flannel and trolled along the surface, is yet able, when blinded, to get his food with great ease. Several blind specimens in the Plymouth tanks were carefully watched by me; and I had no difficulty in deciding that it was by smell alone that they found their food. Their conduct was exactly such as was seen in the smell-feeders, to which I shall presently refer.

"Again, the cod (*Gadus morrhua*), which Bateson puts among the sight-feeders, is generally believed—and with good reason, I think—to feed more by night than by day; which suggests that it, too, not only tests its food, but actually hunts by smell.

"Lastly, in this connection I would state the results of my experiments. I worked with a number of fish, and always with the same success; but I shall here only refer to one case—that of the dabs (*Pleuronectes limanda*). That they were sight-feeders was evidenced by their behaviour when I lowered a closed tube full of water, and with a worm in the middle of it, into their tank; time after time they bumped their noses against the glass at the very spot where the worm was situated. That they could also recognise the smell of food, apart from seeing it, was demonstrated in various ways. First, if instead of a closed tube, as in the last-mentioned experiment, one open at the bottom was used, after a short interval the nosing at the part where the worm was seen ceased, and *the lower end of the tube*, from which, doubtless, worm-juice was diffusing, was vigorously nosed. If, again, instead of putting worms into a tube I placed a number of them in a closed wooden box with minute apertures to let water pass in and out, there was a similar excitement produced, and the dabs hunted eagerly in every direction. When water in which many worms had lain for some time was simply poured into the tank through a tube that had been in position for several days, and by a person who was out of sight of the dabs, the results were most marked. In a few seconds hunting began, and in their excitement the dabs frequently leapt out of the water, apparently at air-bubbles, and on one occasion one even cleared the side of the tank, which was about two inches above the water, and fell on to the floor of the aquarium. Yet there was nothing visible to stimulate this quest."

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## The Thyroid Gland.

In the August number of our excellent contemporary, the *Medical Magazine*, Mr. Lorrain Smith has an interesting paper on the thyroid gland, which, as everyone knows, is the fashionable subject in current Pathology. It has been shown recently that many glands in the mammalian body have, at least, a double function: they form their special secretion as the liver, for instance, forms bile, or the kidneys remove nitrogenous material from the blood; but in addition they have some part to play in the general economy of the organism. Their loss by disease or excision not only prevents the formation of the special secretion, but disturbs the general economy of the body. The thyroid is a gland limited apparently to the second form of activity. This activity is at present being sought for: some results go to connect it with the regulation of heat; others with the general metabolism of the body. So far physiologists have not paid much attention to the comparative anatomy and development of the thyroid, but, without question, this side of the subject will raise problems of very general interest. It is, for instance, most probable that the thyroid was originally a slime-secreting organ correlated with the

mode of life of pelagic forms: it secreted slime to gather together the small organisms in the intaken sea-water. The gradual change of function of any organ is a biological problem of vast interest, and here seems a case to be dealt with by the morphologist and the physiologist hand in hand.

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## The Polynesian Islands.

Under the title of *Flora of French Polynesia*, M. Drake del Castillo gives a systematic account of the Flowering Plants and Ferns which grow spontaneously or are generally cultivated in the Society, Marquesas, Pomoton, Gambier, and Wallis Islands. The Society Islands are by far the most important, whether we consider extent, population, and productions, or flora, and of these Tahiti holds the first rank.

With but slight differences, the climate of French Polynesia is the same as that of the other Polynesian islands. In the Society Islands, the mean annual temperature is a little above 75° F. The year is divided into two seasons of almost equal duration, determined by the direction of the wind. From August to October a S.E. wind, called *Maarama* by the natives, prevails, and the weather is rather dry, though storms are not rare. A N.E. wind rules from November to March, and rains are frequent. However, the difference between the two seasons is not strongly marked, and it rains more or less during the whole year. The climate is moister in the high valleys than near the shore, and there is a corresponding difference between the vegetation of the two regions, while the eastern side being more exposed to the storms brought by the S.E. winds, the limit of the humid zone is lower than on the western side.

The Marquesas are drier than the Society Islands. These conditions of climate necessarily produce a luxuriant vegetation, which is, however, more brilliant than varied, and remarkable more for the development in number of individuals than species. The poverty of the flora chiefly consists in a want of special forms, a characteristic of small islands. Annuals are poorly represented, almost two-thirds of the whole vegetation consisting of small perennial shrubs, while trees and large shrubs constitute another third. This predominance of shrubby plants is explained by the nature of the soil. On the abrupt sides of the valleys, woody plants with short stems and vigorous roots, and ferns with creeping rhizomes, are almost the only kinds which can profit by the scanty support afforded by the soil. Tall trees occur for the most part only in the lower valleys or in the ridges. Feebler plants can only get on by the help of their neighbours, which they use as supports or hosts; hence it comes that 15 per cent. of the vascular plants are climbers, parasites, or pseudo-parasites. The few herbs live on the borders of streams or dry hills. Owing to its rocky nature and steep slopes in the mountainous islands, the soil retains but little of the large amount of water it receives,

while the narrow deep valleys keep the air constantly charged with moisture. Thus, if the roots absorb little, the leaves exhale but little, a fact which explains the chartaceous or coriaceous nature of the foliage of many species.

M. Castillo remarks that the flora of French Polynesia is a poor one, containing, according to our present knowledge, only 588 species of vascular plants distributed among 79 families, and representing 262 genera. They are classed under three headings. First, species confined to the islands, numbering 161; secondly, species distributed throughout Oceania, 123; and thirdly, comprising a half of the flora, or 297 species, those which are also found in the Indo-Malayan region. The predominating families are, Ferns, Leguminosæ, Orchids, Rubiaceæ, Grasses, Cyperaceæ, Euphorbiaceæ and Urticaceæ. The predominance of ferns—with their light and easily-carried spores—over the less easily dispersed flowering plants is a well-known characteristic of insular floras, consequently it is not surprising to find this group represented by as many as 142 species, or nearly one-fourth of the whole. There are 37 species of Leguminosæ, 34 Orchids, 31 Rubiaceæ, and 30 Grasses. A *Freycinetia* is specially mentioned as very plentiful and covering large areas, and along with the plaintain (*Musa paradisiaca*), a plant of a very different type, recalling the vegetation of a Malay or East Indian jungle.

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## Lake Vegetation.

In two recent numbers (nos. 54 and 55) of the *Revue Générale de Botanique*, A. Magnin discusses the vegetation of the lakes of Jura. After exploring a number of lakes, the regularity with which the plants are distributed becomes very striking. Passing from the shore to the centre, distinct zones are noticed somewhat as follows.

1. A littoral zone of reeds (*Phragmites communis*) and bulrushes (*Scirpus lacustris*), which stand out of the water. Sometimes the two plants are more or less intermingled, but usually the reeds alone line the shore, forming the *Phragmitetum*, extending to a depth of 2–2½ metres, while the bulrushes form a second ring, or *Scirpetum*, to a depth of 3 metres, a mixed zone uniting the two. Sometimes the order is inverted through the formation of a bar beyond the shore, the reeds installing themselves in the shallower water. Besides these, where the water is shallow, sedges, *Phellandrium*, *Equisetum limosum*, and others are observed, as well as some plants with swimming leaves, like *Polygonum amphibium*, the white water-lily (*Nymphæ alba*), and *Potamogeton natans*.

2. Beyond the bulrushes plants with stems standing out of the water are no longer found, but only those with swimming or floating leaves. Especially characteristic of this zone is the yellow water-lily (*Nuphar luteum*), which occurs in almost all the lakes of the Jura, frequently forming a large continuous girdle, or great patches, from the limit of the scirpetum to the edge of the first slope to a depth of 3–5,

usually 4, metres, and constituting the *Nupharetum*. Sometimes it is associated with species of the preceding zone, chiefly *Potamogeton natans*, while the water milfoil (*Myriophyllum*), *Potamogeton perfoliatus*, and others of the following zone begin to appear.

3. The third zone, or *Potamogetonetum*, is formed by plants different according to the lake; sometimes pondweeds, chiefly *Potamogeton perfoliatus*, then *P. lucens*, or *P. crispus*, less often *Myriophyllum spicatum*, or still more rarely the mare's-tail (*Hippuris vulgaris*), all bearing their leafy and flowering branches at or near the surface, on stems 4 to 6 metres long. Their rootstocks occupy the edge of the slope, below the yellow water-lily, at a depth of 4–5 metres, descending to a depth of 6 and 7 metres. Here also lives the hornwort, *Ceratophyllum demersum*, which, after wintering in depths of 3 to 6 or even 8 metres, breaks loose and becomes quite free, floating like the bladder-wort (*Utricularia vulgaris*).

4. The fourth or deep zone, the *Characetum*, is formed by plants which never reach the surface, but remain always fixed to the bottom at a depth of 8, 10, and 12 metres. Such are the numerous Characeæ, *Naias major*, and the mosses *Fontinalis antipyretica* and *Hypnum giganteum*.

In deep lakes with rocky borders, plant-life is absent or represented only at points where erosion or a falling has occurred by tufts of reeds or bulrushes, associated or isolated, and sometimes followed inside by the yellow water-lily and *Potamogeton perfoliatus*. The turf-lakes with abrupt, sometimes sharply-sloping margins differ in having a very narrow littoral zone, consisting of the Bog-bean, (*Menyanthes*), the reed mace (*Typha*), *Cladium*, the reed, and the bulrush, while the yellow water-lily reaches almost to the edge. The floor is generally carpeted, to a great extent, with myriophyllums and charas, together with, in some lakes, the hornwort, the two mosses above mentioned, *Nitella* and others.

Shallower lakes with muddy or marshy banks with but a slight incline, and alternately swamped or exposed, show a very variable vegetation. In some can still be seen the littoral zone, the yellow water-lily zone, and a bottom covered with myriophyllums and charas; in others mare's-tail and water milfoil clothe the whole lake, while scattered patches of *Nuphar*, *Polygonum amphibium*, and others are seen at the surface.

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## Classification of Conifers.

The most recent issue of the Linnean Society's *Journal* (vol. xxx., no. 205) contains some valuable notes by Dr. Masters on the genera of the two large orders of Gymnosperms, Taxaceæ and Coniferæ. The observations are the outcome of a comparative examination of the morphological characters of all the genera as far as was possible in living plants, while the available museum and herbarium specimens have been studied as well as the literature. Dr. Masters is well-

known as an authority on the subject, and the notes are a valuable addition to the work he has already published on Conifers and Taxads. The orders, tribes, and genera are carefully defined, and the history of each genus as well as the morphology of the vegetative and reproductive organs is discussed. Constant reference is made to the schemes of arrangement of the older writers, and, more recently, of Eichler, Van Tieghem, and others, but, in the main, the author has followed Bentham's plan, as elaborated in the "Genera Plantarum." Several deviations from the last arrangement claim to be "more in harmony with ascertained facts, or with more recently-acquired information." Thus, the Oriental genus *Cephalotaxus*, which Bentham places among the Taxodineæ, forms with *Gingko* and *Torreya* a distinct tribe, Salisburineæ, of Taxads. The drupe-like seed, and the germination, recall those of *Gingko*, and another point which separates both from all other Conifers is the presence of a resin-canal in the centre of the pith, observed by Van Tieghem. That author is also responsible for the genus *Stachycarpus*, a name originally proposed by Endlicher for a section of *Podocarpus*, to include those species where the peduncle does not become fleshy. The characteristic of the new genus is the arrangement of the fruits on a loose spike, the axis of which does not become fleshy. There are also distinctions in minute structure.

An important deviation from Bentham's arrangement is the splitting of the genus *Callitris*. In the "Genera Plantarum" this includes a species from North Africa, a few from Southern and Tropical Africa, and a number from Australia. Dr. Masters separates the North African plant as a new genus *Tetraclinis*, distinguished by its flattened (not triangular) Salicornia-like branches, and its solitary female cones, the axis of which is not prolonged beyond the four scales. Endlicher's *Widdringtonia* is revived for the other African and Madagascar species with angular branches, panicled female cones, and four unequal scales, while *Callitris* is reserved for the Australian species, the genus *Frenela* of Mirbel differing from *Widdringtonia* in having six scales in the female cone. Another genus of Endlicher, *Glyptostrobus*, included by Bentham, following Brongniart, in *Taxodium*, is restored.

In the tribe Abietineæ, which includes the spruces, cedar, larches, firs, pines, and their allies, Dr. Masters recognises the same genera as Bentham, except that Carrière's *Keteleeria* is revived on the ground of Professor Pirotta's recent observations on the male flowers and other remarkable characteristics. It contains, besides the original *Abies Fortunei*, a few other Chinese species.

The paper concludes with an account of the geographical distribution of the genera. China and Japan are remarkable for the number and variety of their Taxads and Conifers; two genera *Pseudolarix* and *Keteleeria* are confined to China, *Cunninghamia* extends to Cochin China, while *Gingko* is of Chinese origin, but widely cultivated in Japan. *Sciadopitys* is exclusively

Japanese. In North America the rocky mountains form a marked dividing line, the species to the west and east being, though closely allied, for the most part different. No true Conifers are found on the Andes, though the range forms, as it were, a continuation of the Rocky Mountains; their place is taken by Podocarps and Taxads working up from the south. *Juniperus*, *Libocedrus*, and *Pinus* are the only true Conifers found both north and south of the Equator, while among Taxaceæ, *Podocarpus* and *Dacrydium* occur in both subdivisions. Junipers, cypresses, spruces, firs, and pines spread across the northern hemisphere from west to east, but there is no such continuity in the south among true Conifers, though *Araucaria* is common to Eastern South America, Chile, Australia, New Zealand, and some of the South Sea Islands.

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In the recent issue of the *Trans. and Proc. New Zealand Inst.* (vol. xxv., 1892) T. Kirk describes the heterostyly, or differences in length of the style or stamens of flowers in relation to reciprocal fertilisation in the New Zealand Fuchsias. Each of the three species, *Fuchsia excorticata*, *F. Colensoi*, and *F. procumbens*, is trimorphic, having a long-styled, mid-styled, and short-styled form. The arrangements for cross-fertilisation are much less complicated than in our own purple Loosestrife (*Lythrum salicaria*) as the long-styled flowers are, at any rate in the first two species, practically female, the pollen-grains being abortive.

The three forms of *Fuchsia excorticata* grow intermixed, usually in about the same proportion, wherever the plant is plentiful, the long-styled form producing the largest quantity of fruit, a fact which, it is suggested, is largely due to the application of pollen from both the mid- and short-styled forms. This assumption is supported by the great reduction in the quantity of fruit on the long-styled form in the few observed cases where one of the others appeared to be absent. The author knows of no instance of the different forms of *F. procumbens* growing intermixed; only one being found in any one locality. This, he says, may well account for the fact that the handsome fruit of this species has not been seen in the wild state. The mid- and short-styled forms are often cultivated, but were never seen in the same garden. Under these conditions although most frequently sterile, they do occasionally fruit. Hence it is inferred that each form of flower is almost sterile with its own pollen.

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Dr. E. v. Halácsy spent last June in a botanical exploration of some of the North Peloponnesian mountains, and his collections may be expected to yield results of interest. The characteristic elements of the different plant formations were noted, and the upper and lower limits of the vegetative zones determined. Of special interest is the discovery of a Berberidaceous plant on the northern

slope of Panachaion, most nearly allied to *Lemtice Altaica* from the Altai mountains, and probably representing a new genus. On Mt. Olenos Dr. Halácsy found above the fir region a girdle of large trees of *Juniperus fœtidissima*. The high mountain flora at the edge of the snow-fields made a gorgeous display, especially in the heights of Chelmos, where *Ficaria Peloponnesiaca*, *Anemone olanda*, Crocuses, Scillas, the endemic violet (*Viola Chelmiani*), and others, formed brilliant carpets, but nowhere were the species found which characterise the Austrian alpine flora.

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Biologists, as a rule, are apt to overlook the physical aspect of the facts and phenomena with which they deal, and it is therefore gratifying to observe the occasional incursion of the Physicist into the Biologist's domain, such as has just been made by Professor Johnstone Stoney. The Professor asks (*Sci. Proc. Roy. Dublin Soc.*, n.s., vol. viii., pp. 154–156): whence comes the vital energy of the innumerable bacilli which are excluded from the direct influence of sunlight, and why are these organisms all extremely minute? There are some bacilli, *e.g.*, the nitrifying bacilli of the soil, which seem to thrive entirely upon mineral food, and, not only so, but perform their functions while completely removed from the influence of the sun's rays. The manufacture of protoplasm and other complex compounds from inorganic materials involves a considerable amount of energy, and the bacilli must somehow obtain this from the surrounding gases and liquids. Professor Stoney regards it as conceivable that the energy may be imparted to the organisms directly by the impact of the more swiftly moving molecules of these gases and liquids; and if that be the case, then the necessity for the excessive minuteness of the bacilli—scarcely more than molecules—is explained.

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Our knowledge of the more minute structure of the Protozoa has considerably increased of late, as the result of improved methods and apparatus for study; and the protoplasm of an *Amœba*, for example, now appears as something far more than mere jelly and granules. It is now possible even to cut sections of these minute organisms and to stain them in such various ways as to distinguish a most elaborate arrangement of parts in many cases. Last year Dr. F. Schütt published in Berlin the result of some studies of *Peridinia*, in which he not only demonstrated the existence of remarkable spaces in the protoplasm, but a true excretory system in connection with them; and another investigator (Professor Greef) supposes that in sections of *Amœba* he can see striations denoting the arrangement to which the contractility of the animal is due. Mr. J. E. S. Moore is now following up these investigations in the Huxley Research Laboratory at South Kensington, and the last number of the Linnean Society's Journal

(*Zool.*, vol. xxiv., pp. 364–368, pl. xxvii.) contains some of his first results. Mr. Moore has examined sections of *Spirostomum* and *Paramæcium*, and detailed descriptions, with beautiful figures, are given. It would be premature at present to speculate as to the meaning of the various structures, but it is evident that a new line of research of great biological importance has been inaugurated.

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THOSE who are interested in the progress of the American theories of mechanical evolution, ought not to overlook Professor John A. Ryder's papers on "Energy as a Factor in Organic Evolution," and "The Mechanical Genesis of the Form of the Fowl's Egg," in the last number of the *Proc. Amer. Phil. Soc.* (vol. xxxi., pp. 192-209). The arguments are ingenious and interesting, but we are by no means convinced that the phenomena of life are such simple, mechanical matters as Professor Ryder and his school suppose.

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M. MAREY is still making progress with his study of the locomotion of animals, by means of photography. He has just published figures of the gait of a snake, a gecko, and a scorpion (*Comptes Rendus*, vol. cxvii., pp. 355–359).

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A RED deer (*Cervus elaphus*) with ten points on each antler has lately been shot by Lord Burton in the forest of Glen Quoich, Scotland. This is apparently the most complex form of antler hitherto obtained in a Scottish specimen.

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IT has long been known that the Tapirs—which are curiously restricted at the present day to the Malay Archipelago and to Central and South America—once had a very wide range throughout Asia, Europe, and North America. There are traces of these marsh quadrupeds in the Miocene and Pliocene rocks of Germany, France, and England, and in the Pleistocene of China; and quite a long series of ancestral types has been discovered in North America. A brief synopsis of all the known extinct forms has just been contributed to the *Geological Magazine* (Dec. 3, vol. x., pp. 391–396) by Mr. Charles Earle, and all who are acquainted with the tooth-structure and foot-structure of Ungulates will read this account with great interest. So far as known, the Tapirs occur a little earlier in Europe than in America, and the teeth of all the extinct forms are somewhat simpler than those of the species now living.

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WHEN will Palæontologists cease to elaborate theories and try to amend long-respected "laws" on the evidence of mere scraps of animals? In a letter we publish elsewhere, Professor Hutton says that the supposed evidence of the occurrence of the New Zealand Moa in Queensland proves to be worthless—that it consists solely

in a thigh bone which is much more like that of an Emu or a Cassowary than that of a Moa. "There is thus no longer any reason for supposing a late migration of Struthious birds between New Zealand and Australia." At the same time (*Proc. Linn. Soc. N. S. Wales* [2], vol. viii., pp. 7–10) Mr. De Vis discusses a second Queensland fossil—the upper jaw of Owen's great extinct wombat, *Phascolonus*—and points out how another guess has been exploded. Some have supposed that the huge broad chisel-like teeth named *Sceparnodon* by Owen were the upper incisors of this animal, but Mr. De Vis's discovery now disproves the supposition. *Phascolonus* has very stout upper incisors, and the animal to which the broad chisel-like teeth belong is as mysterious as ever. It is a distinct advantage to have accurate descriptions and figures of fragmentary fossils, and it is often desirable to assign to them provisional names; but literature is already sufficiently burdened, and may well be spared the additional incubus of elaborate guesswork and prosy prophecy.

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In reference to general principles touched by this kind of Palæontology, we desire to add a word of warning to students who come across Professor H. G. Seeley's note on a supposed reptilian tooth with bifurcated root in the last number of the *Ann. and Mag. Nat. Hist.* (ser. 6, vol. xii., p. 227). It is a generally accepted principle that the teeth of reptiles are distinguished from those of mammals by having invariably a simple—not bifurcated—root. There is, it is true, not much difference between the longitudinally grooved root of certain reptiles and the double-rooted premolars of typical mammals; but there is no certain exception to the rule as yet on record. Now Professor Seeley describes what he terms an abnormal tooth of *Nuthetes*, an extinct reptile from the Purbeck formation; and if the determination be correct, the discovery is of great interest. We must, however, point out that there is no evidence whatever that the tooth described does belong to *Nuthetes* or to any other reptile; it was found isolated, and in a deposit where mammalian remains occur. Moreover, it appears to us much like the upper canine tooth (with bifurcated root) of the little insectivorous mammal *Triconodon* found at the same place. There is thus so much uncertainty in this determination that the good old law remains unaffected, and we only regret that such comparatively worthless fragments should add further to the literature of palæontological guesses.

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In the same paper Professor Seeley incorporates an interesting observation which could not be suspected to occur under the published title, and we therefore extract it. He remarks how the little Purbeck reptiles, *Nuthetes* and *Echinodon*, seem to be dwarfed representatives respectively of the Megalosaurian (carnivorous) and Stegosaurian (herbivorous) Dinosaurs—animals which in other formations are many feet in length. Sir Richard Owen long ago pointed out how

almost all the crocodiles from the division of the Purbeck Beds in question were dwarfs, and discussed the curious association of these predaceous creatures with the well-known dwarf mammals of the period. The further recognition of dwarf Dinosaurs existing at the same time is thus of especial interest. All of these are land- or freshwater-animals forming one fauna at one period in one and the same region (present Isle of Purbeck), and it seems very difficult to explain so remarkable an occurrence. The dwarfing of fishes is much more comprehensible from the circumstances of their environment; and it is curious to note that in one Purbeck stratum in Wiltshire all the fossil fishes are diminutive representatives of types that are comparatively large elsewhere.

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It is curious to note how widely distributed are some of the great reptiles of the Jurassic period. The marine Ichthyosaurs, for instance, have been found in most parts of the world. Remains of one species were brought home by Sir E. Belcher from the islands between West Cornwall and North Devon in the Arctic Regions; and twenty years ago vertebræ and paddle bones of two other species were discovered by Baron Nordenskiöld in Spitzbergen. Throughout Europe and in North America remains are abundant; and detached vertebræ have been found near Mombasa in East Africa. Other evidence of these saurians has been discovered in Australia, and even in New Zealand as far south as the 45th parallel of south latitude; and now comes the welcome announcement of the occurrence of an Ichthyosaur at two localities in South America (W. Dames, *Zeitschr. deutsch. Geol. Gesell.*, 1893, pp. 23–33, pl. i.). Some dorsal vertebræ with associated fragments from the Tithonian Formation of Cienegita, in the Argentine Republic, are described by Dr. Dames as *Ichthyosaurus bodenbenderi*; and he records the discovery of two vertebræ and a paddle bone of the same genus from the Andes of Chili.

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It is remarkable how new fossils are continually being discovered even in the most thoroughly explored strata. The fossil fishes of the Caithness flagstones have been diligently collected for seventy years, and yet it is only during the past summer that a specimen of the typically Lower Devonian genus *Cephalaspis* has been met with in these rocks in the neighbourhood of Thurso. The fossil was described by Dr. R. H. Traquair at the recent meeting of the British Association, and is now in the Edinburgh Museum. A full account of it is promised in a forthcoming volume of the *Proc. Roy. Phys. Soc. Edinburgh.*

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It is interesting to note that two species of Ostracoda found in the Bear-River Formation (Upper Cretaceous) of Wyoming, have been identified, by Professor T. Rupert Jones, with forms discovered in this country—the one being *Cypris purbeckensis*, Forbes; the other

a variety of *Cypridea tuberculata*, Sow., named *wyomingensis* by Professor Jones (*Geol. Mag.*, Sept., 1893).

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A. Hollick claims to have discovered a fossil palm-leaf in the Cretaceous formation at Glen Cove, Long Island, a discovery of some significance, as "other fossil leaves associated with it show the geological horizon to be the equivalent of the Amboy Clays of New Jersey and other middle Cretaceous strata in America and Europe, from none of which have palms been definitely recorded." The leaf, which is small, fan-shaped, and about three inches across, is made to rank as a new genus, *Serenopsis*, the name indicating its supposed ancestral relation to the Southern Palmetto (*Serenæa*).

The genus was established in the April number of the Bulletin of the Torrey Botanical Club, and the August number of the same journal contains some further information obtained from a second specimen recently found, and in a better state of preservation. It may be well to mention that Professor Lester Ward, whose opinion on the specimens was asked, does not think it a palm, "but suggests that it is more likely to be allied to the organisms which have been called *Williamsonia*, whose affinities are exceedingly problematic."

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It has long been uncertain whether the typical Carboniferous plant, *Lepidodendron*, occurs so low as the Devonian rocks in Australia. Messrs. Pittman and David now seem to have settled the question by its discovery on Mt. Lambie, New South Wales (*Proc. Linn. Soc. N.S. Wales* [2], vol. viii., p. 121).

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The idea that there were high alps with glaciers in the neighbourhood of central France when the Coal-measures of that region were deposited, finds favour with another geologist who has been studying the curious breccias and barren strata in the series (A. Julien, *Comptes Rendus*, vol. cxvii., 1893, pp. 344–346). The result is very interesting, and the conclusions seem far from improbable; but we are pleased to observe that Mr. Mark Stirrup, in his latest paper on the boulders from the Lancashire Coal-measures, does not attempt to infer the occurrence of glacial conditions from the comparatively insignificant phenomena in his region (*Trans. Manchester Geol. Soc.*, vol. xxii., 1893, pp. 321–331). Mr. Stirrup confines himself to the careful observation of the facts of the case, and Professor Bonney has minutely examined many of the pebbles from the petrologist's point of view. Besides the well-known quartzites, Mr. Stirrup has now discovered examples of granite, gneiss, and probably felsite, but all these rocks are seen to be much decomposed when examined microscopically. Professor Bonney thinks they could all probably be "matched" in Scotland, though nothing definite can be stated as to their derivation. It is now evident

that extraneous pebbles occur in the coal-seams and associated shales in many coal-fields both in Europe and America.

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It seems to be proved that the Northern European sea of the Eocene period was separated from that of the Mediterranean area by a land barrier extending across France and Northern Germany. Hence it has been noticed that the fossil marine organisms to the south bear witness to highly favourable conditions for growth, while those in the north seem to have been stunted by the chilling influences of cold currents. This phenomenon is still further exemplified by a recent study of the British Eocene Bryozoa (J. W. Gregory, *Trans. Zool. Soc.*, vol. xiii., pp. 219–279, pls. xxix.–xxxii.), which are numerically small, both in species and individuals, compared with the wealth of forms that inhabited the contemporary seas of the Mediterranean basin. The northern species are also remarkably dwarfed. The land barrier seems to have been destroyed in middle Eocene times, but the conditions were not seriously modified until later. Incidentally, Dr. Gregory remarks that the species of Bryozoa are not so long-lived as some palæontologists suppose; he has no faith in the identification of Cretaceous species with any of those of existing seas.

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The "parallel roads" of Glen Roy are still inspiring those who have the opportunity of studying glacial lakes, and Dr. Robert Munro last year kept the much-discussed Scottish phenomenon in mind while visiting Norway. He found a remarkable example of a glacier lake, formed by a branch of the Hardanger-Jökul, near Eidfiörd, and has now communicated a valuable detailed account of it to the Royal Society of Edinburgh. The paper appears in the last part of the Society's *Proceedings* (vol. xx., pp. 53–62), accompanied by a map. Dr. Munro concludes that the evidence of the existence of glacier lakes furnished by the so-called "parallel roads" of Glen Roy, corresponds, in its minutest details, with the facts observed in his study of the recent lake in Norway.

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Various hypotheses have been started with reference to the Dartmoor Granite, but the old view of De la Beche, that it was intruded among the Devonian rocks and Culm-measures, into which it sent veins, is supported in a recent paper by Lieut.-General C. A. McMahon (*Quart. Journ. Geol. Soc.*, vol. xlix., pp. 385–395). Such veins have been observed by the author near Lydford, on the western flank of Dartmoor. He also drew attention to the pseudo-bedding of the granite, remarking that this conforms closely to the present slope of the surface of the hills. In his opinion it may be attributed to the action of the sun's heat and frosts, in rupturing the superficial portions of the granite, and thereby producing the pseudo-bedding and joints.

A new Cornish mineral has been determined by Mr. H. A. Miers (*Nature*, August 31). It is a hydrated sulphate and chloride of copper and aluminium, and it occurs in brilliant and translucent crystals of a deep emerald-green colour. In external characters and composition it is identical with Spangolite, of which only one example was previously known, and that was described in America, and obtained from a man living in Arizona. Mr. Miers hopes that search will be made among old collections, and especially upon copper ores from the St. Day Mine, near Redruth, for this is the locality whence this new specimen was obtained.

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The report of the Director-General of the Geological Survey for the year 1892 occupies twenty-nine pages in the last Report of the Science and Art Department, and it presents a much fuller statement of the scientific work of the staff than has hitherto been made. Many new facts are thus for the first time announced, so that the volume is one to be noticed by compilers of geological records. There are fresh facts in reference to enormous boulders in the Drift, and on the occurrence of Eskers or Kames in the West of England. There are notes on the Hampshire Tertiary strata, on the Chalk and other Cretaceous rocks of England, and on the Jurassic rocks of England and Scotland. There is the latest official classification of the Devonian rocks, and many remarks on the volcanic and metamorphic rocks of Britain generally. Folds and thrust-planes are noticed, not only in the Highlands, but in the regions of Purbeck and Weymouth.

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The Report of the Royal Commission on London Water Supply has been presented to the House of Commons. The Commissioners are strongly of opinion that the water, as supplied to the consumer in London, is of a very high standard of excellence and of purity, and that it is suitable in quality for all household purposes. With respect to the quantity of water which can be obtained within the watersheds of the Thames and Lea, they are of opinion that a sufficient supply to meet the wants of the metropolis, for a long time to come, may be found. From the River Lea, with adequate additions to the present storage, 52½ million gallons may be taken daily; and by the construction of storage-reservoirs in the Thames Valley, at no great distance above the intakes of the companies, it will be possible to obtain an average daily supply of 300 million gallons, without taking in any objectionable part of the flood-water. At present not half that amount is taken, but the scheme suggested can be carried out progressively to meet the increasing demands for water. A large supply of water might also be procured from the Chalk area in the basin of the Medway. In the opinion of the Commissioners a sufficient quantity of water to supply 35 gallons a head to a population of 12 millions could be obtained; and the population of Greater London

is not likely to exceed that amount during the next 50 years. The Report will therefore be read with satisfaction by those interested in London Water Supply; it is signed by Lord Balfour of Burleigh (chairman), Sir George Bruce, Sir Archibald Geikie, Professor James Dewar, Mr. George H. Hill, Mr. James Mansergh, and Dr. W. Ogle.

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We learn from the *Zanzibar Gazette* and recent letters that in his recent ascent of Mt. Kenia, Dr. J. W. Gregory was stopped at a point 2,000 feet below the summit by a cornice of snow, which could not be surmounted except by a properly-equipped party of climbers. He established a baggage camp at the foot of the mountain, a reserve camp at 11,000 feet, and an upper camp close to the snow-line. The greatest difficulty was experienced in passing through the zone of bamboos, where advantage had to be taken of elephant paths. The succeeding forests proved very dense, damp, and cold, and it was a relief to emerge into the higher pastures. Dr. Gregory collected plants and insects, examined some large glaciers and determined their former much greater extension, and mapped the S.W. side of the mountain.

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We regret to hear that during his exploration of the now active volcano, Adjuma-yama, near Fukushima, in Japan, a member of the Japanese Geological Survey lost his life, and that of his assistant, owing to a sudden explosion. Mr. S. Miura, the deceased geologist, was formerly science master in the Normal School at Saga in S. Japan, and had only recently joined the Survey. The mountain can now be visited with safety, as the explosions occur at regular intervals, morning and evening.

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One could scarcely accuse a writer so well-informed as Mr. Joseph Hatton of carelessness but that under the heading "Cigarette Papers" in the *People* of September 3 appeared an extraordinarily absurd account of a gigantic squid. This is referred to as an "up-to-date fish of the cephaloptera species," with "long scaly arms," or as the writer puts it in another paragraph, "forty-foot antennæ." The remarks are made with regard to an account of the appearance of one of these gigantic molluscs to some sailors in the Gulf of Mexico, and whether or not the nonsense occurs in the American newspaper account, a reference to any encyclopædia would have enabled the English writer to avoid repeating so strange a tissue of absurdities.

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The Anthropological Institute issued in August an "Index to the Publications of the Anthropological Institute of Great Britain and Ireland (1843-1891), including the Journal and Transactions of the Ethnological Society of London (1843-1891): the Journal and Memoirs of the Anthropological Society of London (1863-1871): and

the Anthropological Review." The volume consists of 302 pp., and is priced at ten shillings. In the main one alphabetical arrangement is followed, but why this is not carried out under the headings "Exhibitions," "Reviews of Books," etc., passes our comprehension. These items are apparently arranged in the order of publication, and the inconvenience of having to wade through 16 pages of index when hunting up the review of a book is obvious. The publication is extremely valuable, and is an example to several other London societies (*e.g.*, Geological and Geographical), whose publications have in part been indexed by Americans. The neglect to provide a general index for journals like the *Annals and Magazine of Natural History* and the *Geological Magazine* too, which has been running now since 1864, speaks little for the enterprise of publishers. In the last instance, we are informed, the work of preparation has been offered gratuitously, but the outlay on printing has been refused. The consequence is that many valuable papers are overlooked or forgotten, as a serious expenditure of time is entailed in searching through the indexes of thirty or forty volumes. They manage these things better abroad, for such publications as the *American Journal of Science* and the *Neues Jahrbuch für Mineralogie* are provided with general indexes at intervals.

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An excellent portrait of the late Mr. James William Davis appears in the current number of the *Geological Magazine*.

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The sixtieth anniversary of the birthday of Baron F. von Richthofen has been celebrated by the issue of a "Festschrift," a volume of 418 pages, illustrated with a portrait and maps. It consists of a series of essays, chiefly on geographical subjects, by the Baron's pupils. The seventieth anniversary of the birthday of Professor J. Victor Carus was marked by the issue of a beautiful portrait with the *Zoologischer Anzeiger* for August 14, 1893.

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The *Mediterranean Naturalist* is now issued only at intervals of two months. The August number contains, among other matter, a plea for the establishment of a Museum of Natural History and Archæology for Malta.

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We are always glad to note the success of any scheme that has for its object the infusion of a scientific spirit into collectors. It is thus a matter for gratification to observe that the small "monthly medium for collectors and students of Natural History" known as *The Naturalists' Journal* has just been enlarged. One enthusiast in the "exchange column" wants some Natural History books or specimens in exchange for a pile of literature of the "railway accident insurance" type. This looks like a conversion.

I.

# The Effect of the Glacial Period on the Fauna and Flora of the British Isles.

THE question of the extent to which the flora and fauna of Britain was driven out of the country, or exterminated during the Glacial period is intimately connected with that of the southern limit of the ice. Geologists in general seem to favour the view that plants and animals were for the most part—if not entirely—expelled, and that our country had to be restocked from the continent. But there are several considerations which point to the possibility of the survival of some part at least of the pre-Glacial inhabitants of the land.

The ice during the Glacial period may have extended as far south as the latitude of London; very possibly it did not extend so far. Our opinion as to the survival or otherwise of our flora and fauna will be influenced by our view as to the southern extension of the ice. Let us take the former view first. This would leave the counties of Kent, Surrey, Sussex, Hants, Dorset, Somerset, Devon, Cornwall, and part of Wiltshire free from ice.

As far as *space*, then, is concerned, we have an area capable of affording an asylum to a considerable number of our plants and animals.

If, however, the Boulder Clay was not formed beneath the ice, then the latter probably did not extend so far south as the latitude of London, and the area fitted to form an asylum for our pre-Glacial flora and fauna increased.

As regards climate, again, it is to be remembered that ice-sheets and glaciers terminate in temperate latitudes when they do not reach the sea—if the latitudes were not temperate they would not terminate; and in connection with existing glaciers we find a temperate flora and fauna in close proximity to the ice, as in Switzerland, the Himalaya, and America. Further, the limits of the ice overlap the limits of temperate life; the latter ascends beyond the termination of the ice, while the former extends into the domains of the latter. Note, for example, how the lines marking the northern limit of forests and the southern limit of the ice overlap in North America.

Such facts lead us to admit the probability that the climate of the south of England during the Glacial period was such as to permit

the continued existence of temperate life and also to enlarge our conception of the area free from ice.

There are other considerations which strengthen this view. The southern limit of the ice, when found, marks its extreme winter extension; during summer a very much larger area of country would be free from ice; for it is to be remembered that, whatever theory of glaciation we accept—since those involving an absolute diminution of solar heat are almost universally discarded—we have to reckon with a summer heat-supply as great as that which we receive to-day. Consider, then, what would be the effect of setting our present summer to work on an England in which the ice reached as far south as London. We may surely assume the melting back of the ice for a considerable distance.

During the most intense Glacial period possible, according to the astronomical theory, our present heat-supply was concentrated into 166 days, but this would probably affect very little its melting powers; and according to Sir Robert Ball's data, the average temperature of the Glacial summer would be 42° F. higher than at present, or about 102° F. Such a considerable retreat of the ice during summer would, of course, tend to make temperate life more possible in the country.

Again, there is evidence which appears to show that the Gulf Stream warmed our western and southern shores as it does to-day.

(1) It has been noted that more southern forms of shells occur in the Drift of the west of England and of Ireland than in that of the east of England.

(2) Glaciation extended further south in North America than in Western Europe.

Therefore, as at the present day there are places considerably north of London with milder climates, so during glaciation there may have been areas fitted to preserve temperate forms of life besides the southern counties before mentioned.

But do the fossils of our Glacial deposits favour the view of the survival of any pre-Glacial plants and animals in our country?

Intercalated with the Boulder Clay are certain sands and gravels containing occasionally organic remains, which those who accept the view of interglacial warm periods claim as evidence of such.

If, as I have endeavoured to show,[1] the evidence for such periods is inconclusive, these organic remains may indicate survival; and it is well-known that such mingling of arctic and southern forms does not necessarily indicate alternations of warm and cold periods. Professor Forbes pointed out an interesting example in his "Fauna and Flora of the British Islands." Cape Cod, in North America, he remarked, forms the line of demarcation between a fauna as northern as that of England during the Glacial period, and another of an aspect perhaps even more southern than that existing at present

[1] *Geological Magazine*, August and September, 1891.

on the coasts of Portugal; and he adds: "For a short space, and but a very short space, the two faunas intermingle."[2]

Take, again, certain parts of the Atlantic Ocean at the present day. A mingling of arctic with temperate forms of life must occur among the deposits there accumulating by means of currents flowing south from colder regions, and the Gulf Stream flowing north from temperate regions.

A similar mixture must occur in the seas north of Siberia, since the great rivers flowing from the far South will carry temperate forms of life to be interbedded with the natives of those icy seas.

An interesting example of the mingling of temperate and arctic forms is recorded from the New Siberian Islands. Here in one deposit were found bones of a long-haired variety of the tiger, along with those of the musk ox, mammoth, etc.[3]

Leaving, however, the doubtful evidence of the so-called "inter-glacial" beds, are there any indications of survival in undoubted Glacial deposits?

In the Glacial fresh-water beds which occur at the base of the Glacial series in Norfolk, *Salix polaris* and *Betula nana* occur along with *Planorbis complanatus*, *Succinea putris*, and *S. oblonga*. The first of these species of mollusca occurs everywhere in England, Wales, and Ireland; the second is fairly common, and the third rare and local *at the present day*. Thus the cold which brought *Salix polaris* did not drive away these temperate forms of mollusca.

In the Boulder Clay of Norfolk, again, the following species are recorded:—*Cardium edule*, *Tellina lata*,* *Cyprina islandica*, *Littorina littorea*, *Mya arenaria*, *Pleurotoma turricula*,* *Pholas crispata*. With the exception of those marked thus *, all occur in the succeeding Glacial gravel; they all, without exception, occur in the preceding Pliocene, while only *Tellina lata* is extinct as a British shell.

*Cardium edule* is now common round our coasts; *Cyprina islandica* ranges as far south as Boulonnais and Cherbourg; *Mya arenaria* occurring in South Greenland, ranges as far south as Rochelle; *Pholas crispata* extends to the North of France; *Littorina littorea* ranges between Greenland and Lisbon; *Pleurotoma turricula* occurs on every part of the British coasts.

As regards Mammalia, *Elephas primigenius* is recorded from the Boulder Clay of the same district, and doubtfully from the Glacial gravel; *Cervus elaphus* is doubtfully recorded from the Boulder Clay.

When we turn to the West Coast of England we find in the organic remains of the Boulder Clay still stronger indications of the possibility of survival. Thus the great majority of shells found in the Boulder Clay of Liverpool are such as are living in British seas to-day; only two, *Astarte borealis* and *Saxicava rugosa*, are distinctly

[2] *Mem. Geol. Surv.*, vol. i., p. 377.

[3] *Mém. Acad. Imp. Sci. St. Pétersbourg*, vol. xl., no. 1. Referred to in "Notes and Comments," NATURAL SCIENCE, March, 1893, p. 170.

arctic, while others—*Venus chione*, *Cardium tuberculatum*, and *Dentalium tarentinum*—are generally restricted to more southern regions.

As far, then, as these particular cases can be taken as typical, we seem to have indications that certain temperate forms of life were able to survive glaciation.

The present distribution of animals and plants in the British Islands has an interesting bearing on the question.

That England only possesses a portion, and Ireland a still smaller portion, of the animals and plants of the Continent, is held to indicate the gradual severance of the land connection by which these countries were restocked after glaciation. Ireland is supposed to have been severed from England while the latter was still connected with France and Belgium. It may possibly be, however, that the evidence of these facts of distribution is rather that England was not restocked by a land connection from the Continent after glaciation. If it can be shown that the missing forms are those most likely to have been exterminated by the cold, or least likely to cross the separating sea, and if, in addition, they are forms calculated to migrate as quickly as those which are common to our country, then there will be an argument against the restocking by a land connection.

The argument of the fifty absent species of mammalia is against the hypothesis of such a land connection, unless it can be shown that there was some other hindrance than lack of time; for the supposition that they had not time will not pass muster: such geographical phases as that implied by the union of the East Coast of England to the Continent by a great plain are usually of vast duration.

Supposing the connection to have lasted 10,000 years,[4] then, to accomplish the journey, they would only need to travel 15, 14, or 3 yards a year, according as they crossed from the Netherlands to Norfolk, from Belgium to Kent, or by the Straits of Dover. It is difficult to think of any species of mammal which, under favourable climatic and other conditions, would not be able to spread more than from 3 to 15 yards per annum. Individuals of most of the species might cross in less than a year. Nor is it easy to believe that any species of reptile would be unable to spread at a much quicker rate than 15 yards a year; and it is to be noted that if the process by which England was united to the Continent was a very gradual one—as such geographical changes usually are—then many species would be half-way across the dividing area by the time the connection was completely established.

With the plants the case is somewhat different, for they are not able to migrate as individuals like animals. Yet when we consider the numerous means at their disposal—rivers running from the Continent towards the central plain, seeds, like those of the dandelion

[4] According to Mr. Jukes-Browne, "it lasted long enough for Palæolithic man to be supplanted by Neolithic man, and for a large number of mammalia to become extinct."—"The Building of the British Islands," p. 297.

and hawkweeds, wafted by the wind, others, like goose grass, burdock, and common Avens, adhering to the fur and wool of animals, others carried by frugivorous birds, or adhering to the beaks and feet of others, etc.—it would be strange if most of them did not migrate more than 15 yards a year.

There is another consideration. The plants which must have crossed the plain on the hypothesis of a restocking from the Continent after the Glacial epoch, include apparently some of the most slowly spreading forms.

While, then, the above considerations seem to indicate that our present flora and fauna did not necessarily migrate across an eastern plain after the Glacial period, they need not be taken as an argument against the existence of such a connection, if that is supported by other facts. There *may* have been such a connection, and yet our plants and animals may have existed pretty nearly as they are now before its existence. If it can be shown, on the one hand, that the absent species are such as ought to have migrated across a land connection, then the present distribution favours the view that our country was not restocked from the Continent after the Glacial period; if, on the other hand, they are species whose line of geographical distribution did not extend so far west as our east coast, or so far north as our south coast, then their absence is no argument either way; and if our country was not restocked from the Continent in post-Glacial times, then the pre-Glacial flora and fauna must have survived to a large extent.

The existence of several species of South European plants in the south-west of Ireland which are absent throughout the rest of Britain also furnishes an argument in favour of survival; for in the absence of a direct land connection between Ireland and the north of Spain since glaciation—for which there is no independent evidence—it is difficult to understand how they can have got there. It seems on the whole a more reasonable supposition that in the warm period preceding the Glacial they were widely dispersed over Britain, and that they were exterminated everywhere, except in the warmest corner, viz., the extreme south-west of Ireland, by the cold. Such was the opinion expressed by Professor Forbes.[5] At the same time the possibility of a post-Glacial migration by other means than a land connection—as, for example, by the carriage of seeds by migratory birds—must not be lost sight of.

A recent discovery which seems to hint that a part of our flora, at least, is a survival, is that of the seeds of *Naias marina* in the Cromer Forest Bed. The only British locality for this plant is Hickling Broad, Norfolk, and hitherto the Forest Bed is the only fossil locality. It would be, to say the least, a curious coincidence if it had been exterminated by the ice and had then re-migrated from the Continent

[5] *Mem. Geol. Survey*, vol. i.

to one spot in Norfolk only. The species is at present a native of temperate and tropical regions of the Old World.

A further indication of the possibility of survival is afforded by the flora of Cornwall and Devon. Like the Iberian element in the Irish flora, the Norman element in that of Cornwall and Devon may have existed there before the Glacial epoch. At least, the precarious foothold of many of the species, and the apparent dying out of some, are not suggestive of species migrating northwards in response to a continually ameliorating climate.

In a paper on "The Climate of Europe during the Glacial Epoch,"[6] Mr. Clement Reid discusses the question of the temperature of Britain during the Glacial Period. Some of his results seem to have an important bearing on the present enquiry. If, as he concludes, the temperature increased *rapidly* towards the south, there would be a greater possibility of survival south of the limits of glaciation. At the same time, a southern England bounded by an ice-foot similar to that of the Arctic regions, as pictured in the same article, does not appear a favourable shelter for any but somewhat hardy forms of plant and animal life. Mr. Reid also arrives at the conclusion that "extensive regions must have been quite uninhabitable for their present fauna and flora." Whether, however, this remark is to be applied to the South of England or not does not appear.

The solution of the problem which forms the subject of this article seems to me an important one, since almost the only evidence of a post-Glacial connection with the Continent is the *supposed* necessity of such to account for our present fauna and flora. To those geologists, indeed, who, on the strength of the evidence of a submerged forest, infer an elevation of the country *en masse*, such a connection—even repeated many times in accordance with the view of many Glacial and inter-Glacial periods—is no difficulty at all; they are as prodigal in respect of earth-movements as those naturalists who would "create a continent to account for the migration of a beetle." To those, however, who shrink from such assumptions, unless supported by strong evidence, and who feel that such oscillations of the land *en masse* are inconsistent with the physics of the earth's crust, the conclusion here indicated may perhaps be a welcome solution of a difficulty.

G. W. Bulman.

[6] Natural Science, vol. ii., August, 1892, p. 427.

## II.

# Some Recent Researches on the Habits of Ants, Wasps, and Bees.

BY most zoologists, the aculeate or sting-bearing Hymenoptera are accorded the highest place among insects; a position warranted not only by the extreme specialisation of their structure and development, but also by the perfection of their family and social instincts, which, from ancient times, have been held up for the admiration and imitation of mankind. Those families of the group in which the social life has been most completely adopted—the ants, and the social wasps and bees—are probably better known to persons not zoologists than are any other insects. Interest in their habits and economy has been greatly aroused by Sir J. Lubbock's well-known work (1), though the leading facts of these insect-commonwealths were long ago carefully observed by Huber.

The orderly and purposeful collective work of ants has often led observers to speculate upon the means of communication between the insects, and they have been believed to converse with each other by motions and contact of their antennæ. Experiments on this subject must be familiar to readers of Lubbock's book. He also investigated the ability of ants to make sounds and to hear. Although supposed organs of hearing have been found in the antennæ and tibiæ of ants, no impression could be produced upon the insects by any sound audible to human ears. Lubbock supposed, therefore, that they can only appreciate notes of a pitch too high to be heard by us. He suggested that certain ribbed areas on the abdominal segments of various species might be organs for producing sound, but he was unable to detect any sounds which these may have made.

These sound-producing areas have recently been specially studied by Dr. Sharp (2). He finds that some of the structures observed by Sir J. Lubbock and others are nothing but the ordinary sculpture of the surface of the segments. In other cases, however, he has no doubt that stridulating organs really exist. At the middle of the base of the dorsal surface of the third abdominal segment, in most species of the Myrmicides and Ponerides examined, he finds a special area traversed by excessively fine lines. In the Ponerides these are modified from the ordinary sculpture of the segment which, on these

spots, becomes finer, closer, and more regular. Indeed, in comparison with the rest of the surface, the stridulating area looks smooth, and a high power and appropriate light are required to make the lines apparent. In the Myrmicides, the lines appear to be not always mere modifications of the general sculpture; in some cases they are described as being "developed on a glassy surface poured out on the ordinary surface." In a species of *Sima* from Australia, the area was found to be divided into two parts, the lines on the one part being much coarser than those on the other, and doubtless enabling the insect to modify the sound produced.

The delicate instrument formed by the fine sculpture is played upon by the specially modified hinder margin of the segment in front, which projects downwards to form a very sharp and even edge. This, drawn across the fine ridges, as the ant moves her abdominal segments, must give rise to an excessively high note. In some cases, at least, the sound is audible to human ears. By moving the appropriate parts, Dr. Sharp has produced sounds with species of leaf-cutting ants (*Atta*). Mr. Wroughton (3) has described the action of a colony of Indian ants (*Crematogaster*) when disturbed; they wag their abdomens, and emit a sound "as if a red hot cinder had been plunged into water." The Camponotides, to which most of our common British species of ants belong, do not appear to possess stridulating organs.

Leaf-cutting ants are familiar to readers of books of tropical travel, and the object of the insects in collecting leaves has been differently explained by various observers. The opinion advocated by Belt, that the ants tear up the leaves in order to make beds on which a crop of fungi may be grown, was confirmed by Mr. Cook, and has recently received more detailed confirmation from the researches of Herr Möller (4), who has studied the habits of these ants in Brazil. Within their nest is a soft, spongy mass, consisting of the remains of leaves, cut into excessively minute fragments and gathered into small heaps to serve as "mushroom gardens," in which a fungus (*Rozites gongylophora*) is cultivated to furnish food for the colony. Four species of ants (genus *Atta*) are found to grow the same fungus. In all cases a small space is left between the "mushroom garden" and the outer wall of the nest, and the beds are never formed in an exposed position. The material collected by the ants naturally contains spores of other fungi than their special kind, and in cultivations prepared by Möller these appeared and developed. In the nests, however, but the one kind is allowed to grow; the ants appear to weed out all but their special food. Over the upper surface of the "garden" are numerous small white bodies, formed by masses of swollen ends of fungus hyphæ, which the ants appear to produce by some special culture. These masses are what the ants feed upon, they never appear to eat the fragments of leaves, which form the fungus-beds. When they take a journey they carry with

them the material of the "garden," even to the smallest fragments, and rebuild the structure in their new abode.

Herr Möller finds that ants of the genera *Apterostigma* and *Cyphomyrmex* are also "mushroom gardeners," but these do not cut leaves to form their fungus-beds. They use instead, wood, grain, or dung. Four species of ants of the former genus cultivate one fungus, and two species of the latter another. Although these fungi all belong to the same group, the ants, even when hungry, are found to refuse disdainfully the food of another genus. Even the species of ants of the same genus, which cultivate the same species of fungus, produce by different methods of culture a difference in the nature of their special food bodies. Those grown by the species of *Atta* are adjudged the prize by their patient observer in the fungicultural contest.

Observations by Herr Möller on another genus of ants (*Acanthognathus*) have been recently recorded by Professor Forel (5). Besides teeth at the extremity of their mandibles, the workers possess, at the base of each of those appendages, a long, curved tooth, directed inwards and downwards. When at work building, or carrying eggs, these ants have their mandibles widely apart, sticking out on either side at right angles to the body. When in this position, the ends of the basal curved teeth just touch each other, and are used by the insect to carry the egg, or a piece of earth for building. When frightened, the ant brings her mandibles sharply together, and the basal teeth are then, of course, crossed.

A most interesting summary of what is known of the various kinds of ants' nests has, also, lately been published by Professor Forel (6), in which he gives the result of observations by himself and other naturalists. Nests made in holes or under stones, nests dug in or built of earth, wood, etc., are described and illustrated from the work of European and exotic ants. The transition between nests built of a papery substance formed of fragments of earth, wood, etc., joined by a secreted cement, to those composed of fine threads drawn out from such cement, and used for binding leaves together, is traced through a succession of species.

Lubbock, in the work already referred to, remarked that it was strange that the exact manner in which new colonies of ants are founded should remain uncertain. He suggested that a young queen, after the nuptial flight, might join an old nest, or found a new colony, either by herself, or with the assistance of a number of workers. Professor Forel, in the paper just mentioned, considers it established that new colonies are founded by solitary females, or by several associated together. Mr. W. W. Smith (7) has lately studied the rise of ant-colonies in New Zealand. The nuptial flight there takes place in March, and the rise of societies was studied through the winter months following. A pair (male and female) or several pairs were found to be associated to form the new colonies; and these always selected sites, under stones or elsewhere, already occupied by

aphids and coccids—the "domestic animals" of the ants. If, through any cause, the aphids left the place, the ants went away too.

To modern naturalists the stages by which the more perfectly social insect communities have arisen furnish a question of great interest. Some facts and suggestions towards the solution of this problem have recently been furnished by Herr Verhoeff (8). He points out that the economy of each social group must be compared with that of the "solitary" genera most nearly akin to its presumed ancestral stock. The affinities of the ants are considered by Verhoeff too doubtful for profitable consideration: he therefore confines his attention to the wasps and bees. The social wasps (Vespidæ) are believed to have arisen from the Eumenidæ, and these from a group of fossorial hymenoptera nearly allied to the Trypoxilidæ. The bees are supposed to have originated from some other fossorial family. Hence the study of the fossorial genera may be expected to throw much light upon the economy of the higher groups. From simply laying the egg in the body of some other insect—after the manner of modern ichneumon flies—was developed the present habit of the fossorial wasps of digging a hole, in which are placed both the egg, and the insect which serves as a prey for the grub when hatched. The habit of catching the prey, and then digging a hole to bury it in, must clearly have preceded that of first preparing the hole and then catching the prey to put in it. As in primitive man, the hunting habit was at first stronger than the home-making. But the simple digging of a hole, once adopted, led on by degrees to the construction of nests of a more and more complicated design. The simple unicellular nest was improved into the linear arrangement of cells, with a common opening, constructed either in the earth or in twigs which may now be observed in species of *Crabro*, *Hoplopus*, *Tropoxylon*, *Colletes*, *Osmia*, *Anthophora*, etc. From this latter, or from the simple cell, is derived the branched style of nest in which the cells open off from the entrance or from a passage leading thereto; such nests are made by many of the solitary wasps and bees, species of *Hoplopus*, *Halictus*, etc. In *Rhopalum clavipes*, which, making its nest in twigs, constructs some of the cells at a very slight angle with the main series, we see how the branched system could arise from the linear. A further advance is seen in the nests not placed in cavities but built of lime or sand upon rocks or walls. The highest forms of nests are the paper structures of the social wasps, and the waxen cells of the bees.

Herr Verhoeff does not believe that the social communities of the wasps and bees have arisen from casual colonies, such as are occasionally observed among solitary species; the bond of the community is to be found in the members being all the offspring of one mother. As in human society, the family has given rise to the state. For the development of a social community three conditions are necessary:—A nest large enough for a number of insects, a close

grouping of the cells, and an association between the mother and her offspring in the perfect state (not simply as larvæ); the last condition will be brought about by the emergence of the older insects of the brood while the mother is still occupied with the younger larvæ or their cells. In a single species of solitary bee (*Halictus quadristrigatus*) these conditions are almost fulfilled, but the earliest young insects to appear are males, and when the females are developed the mother dies. For the formation of a community, of course, the mother must co-operate with her female offspring, and this *Halictus*, therefore, just fails to develop the social habit. It gives, however, a sufficiently strong clue to the origin of that habit.

The difference in habit between bees and wasps gives rise to another interesting suggestion by Herr Verhoeff. The bees, being vegetable-feeders, are more disposed to live peaceably in communities than the wasps. But there is no contact between the mother and larva; a supply of honey and pollen is placed in the cell, the egg laid, and the cell closed up. The wasps, on the other hand, are mostly insect-feeders, and might be expected, therefore, to be of too warlike a disposition to form well-ordered republics. But their mode of life is such as to develop family affection; the mother, after laying the egg, goes in search of prey, and from the fossorial habit of placing alongside the egg a paralysed insect to serve as food for the larva, and then closing the cell, has arisen the custom of feeding the larva throughout the preparatory stage, and only closing the cell during the pupal period.

The nests and habits of many species are described by Herr Verhoeff in detail. Some notes on the wintering of Hymenoptera are of special interest. The winter is passed by various species in all stages, except the egg and the feeding-larva. Curiously enough, insects of the social species are generally found singly in the winter, while individuals of the solitary species gather together in the cold season, as do also some ichneumon flies. A number of males and females of a species of *Ceratina* were found in a bramble stem which they had hollowed out to serve as winter quarters. A winter colony of females of *Halictus morio* (the males of this species die in autumn) was found in a forked tunnel opening under a large stone. In was in early spring that these insects were observed, and, while a number of the bees were gathered together at the end of one branch of their burrow, others were apparently making their way out of the winter retreat.

The habits of some bees (*Trigona*) at Trinidad have been lately described by Mr. Hart (9). These insects have no sting, and consequently adopt special means for defending their nest. The only entrance is by means of a tube which is constricted at intervals so that only one bee can pass. Each of these constrictions would serve as a fortification where a foe might be held at bay. Moreover,

at night the bees entirely close up the tube with a sheet of wax, leaving only a few minute openings for ventilation.

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Geo. H. Carpenter.

## III.

# The Recent Plague of Wasps.

THE remarkable summer just past has been signalised by such a profusion of what is perhaps the most generally unpopular insect pest, that a brief review of the facts may be of interest. Of the seven British species of *Vespa*, no fewer than five appear to have been more than usually abundant. So early as the beginning of June both personal observation and reports from reliable witnesses showed *V. sylvestris* and *V. novegica* to be well established and in possession of strong nests. During the earlier portion of the summer these two tree-wasps were, in S.W. Surrey, far more abundant than the ground-wasps, *V. vulgaris* and *V. germanica*, which, however, were in full force by the middle of July, while *V. rufa* was found more commonly than in previous years, though it could not be said to be numerous, at least from my own information. It is much to be regretted that the letters and complaints in the daily newspapers contain no hint as to the species of wasps abounding in various parts of the country, though the context would in most cases point to ground-wasps. Nevertheless, Mr. Lowe (*Nature*, vol. 48, p. 437) speaks of "the tree-wasp" having had many nests in Monmouthshire, so that we may reasonably conclude that the season has been favourable to tree- and ground-wasps alike throughout England. Of the somewhat mysterious *V. arborea* I have heard nothing, excepting the capture of two females in Ireland (*Entom. Monthly Mag.*, July, 1893, p. 166). It is very singular the hornet, *V. crabro*, has hardly been reported abundant anywhere. I cannot help thinking that this species is decidedly on the wane and disappearing from our islands. Popular observation on the point is well-nigh worthless, for many so called "hornets" prove on inspection to be large female wasps. It would be of great interest to ascertain from all counties whether the "red hornet" has or has not become appreciably scarcer during the last fifteen or twenty years.

Turning now to the causes of this "plague," I have no hesitation in ascribing it almost entirely to a remarkable series of meteorological conditions advantageous to wasp life. Speaking of observations made by myself, which in the main agree with those taken elsewhere, it is noticeable that after March 23 there was no frost registered by the screened thermometer, excepting slight ones on the nights of April 13, 14, 15. Again, with regard to rainfall, from March 1 to June 30 the total amount registered was only 2·38 inches on 27 days,

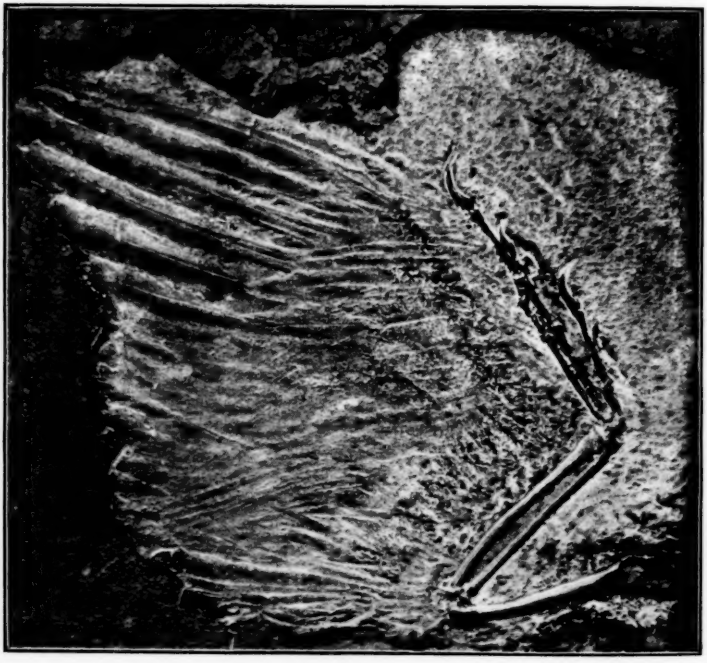
and on no one day throughout the four months did the fall exceed 0·34 inch.

It is not easy to conceive weather better suited than the above to the well-being of wasps. The impregnated females emerge from winter quarters early in the year, generally making their first appearance during March. The occurrence of frost, snow, heavy rains, and the like during April or even May, cannot fail to destroy enormous numbers of females and their first few grubs lying in the small nests built of delicate "wasp-paper" by the queens themselves. Severe weather setting in after the commencement of the nests must be fatal to tree- and ground-wasps alike. This year after the severe weather of early spring had once gone it never returned. Hence the females, tempted from hibernation by warmer days, met with no disaster either in their persons or their habitations, and thus an unusually large number of nests became successfully established. Further than this, the mean temperature for the summer months has been decidedly above the average, and many animals and flowering plants made their appearance several weeks sooner than is customary, thus affording a plentiful supply of food material to the omnivorous worker wasps. The drought appeared to affect the wasps in two ways, advantageously by causing great and rapid increase of Aphides whose secretions the wasps keenly appreciate, in addition to favouring the multiplication of flies, earwigs, etc., on which wasps to a great extent subsist; detrimentally by rendering it difficult to supply an adequate amount of moisture to the growing grubs. In some nests which were taken by chloroforming, I found numerous grubs shrivelled up and many of the wasps captured in the open were unusually small. This lack of water was very clearly shown by the way in which wasps swarmed round plants which were regularly watered so that the leaves and earth were constantly moist; in such cases wasps could be seen at all times of day greedily sipping up drops of water, or getting it from the wettest earth beneath the leaves.

From an economical point of view, a word in favour of the much-abused wasp is due. As scavengers, wasps undoubtedly confer great benefit upon us; the rapidity with which they remove the flesh from dead animals requires to be seen to be believed. On several occasions I exposed dead mice in order to notice the ravages produced by the wasps, and in each instance every particle of soft material was completely removed from the bones in the course of two days.

It is never wise to prophesy, but there are indications of what may be expected next year; the drones and young females have now been on the wing for some time, having appeared earlier than usual. This renders it probable that many females will be destroyed before they search out convenient spots for hibernation, so that already the balance is probably being readjusted, and unless the spring and early summer of 1894 prove repetitions of this year, there is no cause to anticipate a recurrence of the plague.

Charterhouse, Godalming. Oswald H. Latter.



PHOTOGRAPH OF LEFT WING OF *Archæopteryx*, from the specimen in the Natural History Museum, Berlin. *(Two-thirds nat. size.)*

## IV.

# Biological Theories.

### VII.—THE DIGITS IN A BIRD'S WING: A STUDY OF THE ORIGIN AND MULTIPLICATION OF ERRORS.

THE plate accompanying this essay is a photograph from nature of the dorsal aspect of the left wing of the specimen of *Archæopteryx*, which is now in the Museum of the Berlin University, and which was found, in 1877, in a stratum belonging to the Upper Jurassic series, near Eichstätt in Bavaria. This photograph is now published because the best known figure of this specimen is characterised not so much by carelessness or by inadvertent error as by wilful falsification. That figure has, however, been copied into some of the best palæontological and geological treatises, and has received the authoritative sanction which insertion in Zittel's "Palæontologie" involves. In controverting views supported by such an authority, and views to be found even in the careful writings of Professor Huxley, and of almost every lesser light who has ever expressed an opinion on the subject, it is hardly probable that I should gain credence if I had given merely a new drawing. The photograph, however, will carry conviction.

In the plate, the humerus, the straight radius, and *curved* ulna, a carpal bone, and three long slender fingers may be seen at a glance. The fingers are all clawed, though the claw itself is not easy to make out: still the form of the ungual phalanx can leave no doubt upon the subject.

The fingers are recognised by everybody as being the equivalents (or homologues) of the digits I, II, and III of the normal pentadactyle sauropsidan fore-limb: this view is fully supported by the relative positions of the three fingers and by the numbers of phalanges—two, three, and four respectively—which they possess.

To the post-axial side (*left* in the plate) of the ulna and of the fingers is seen a wing in a marvellously perfect state of preservation. In the original, even the barbules of some of the quills are recognisable, and the barbs are easily seen even in the photograph. Of the quills, there are sixteen or seventeen (possibly even one more). The first seven are *primary* quills, and these are of chief importance to us for the present purpose. To merely state that the fourth is straight, and

that the remainder are very nearly so, would not be sufficient. I must beg the reader to lay upon the plate the *straight* edge of a piece of paper, or a rule, and to see for himself that this is true, and that the concavity of the curvature of every primary quill is turned towards the middle one, *i.e.*, the fourth. The hinder ones are curved—but only *very* slightly—in such way that the concavity of the curve is turned forwards. If the line of the rhachis of each individual primary quill be traced onwards, a pretty accurate idea may be obtained as to where these *primary* quills were inserted.

In the Steinmann-Döderlein figure, which is spreading like a plague in modern books, those hinder primary quills which the photograph shows to be curved slightly *forward* (*i.e.*, with the concavity forwards), are represented as curving *strongly backwards*—curving, that is, through an angle of more than forty degrees! The distinction between primary and secondary quills is abolished, and all but the first two or three are represented as attached to the ulna! The second (or ? first) quill is represented as the longest, and the fourth, which is seen in the photograph to be the longest, is drawn much shorter than this, or rather as falling short of it at the tip. The continuation of the quills in a wrong direction as far as the ulna involves, in the case of the third one, the representation of the feather of nearly double its true length.

Nor is it difficult to guess how the originators of the figure came to draw it thus falsely. Inadvertent error on the part of the engraver is out of the question. The drawing is not to be described simply as erroneous, but as deliberately falsified. The uselessness of *strongly*-curved feathers for flight may not have occurred to the authors, but the absurdity of supposing (as many do!) that the three long, *slender*, and especially *weak-jointed* fingers could bear the torsional stress to which they would be exposed during flight if they supported those large quills, appears to have occurred to them and to have led them to avoid the absurdity of this, the everyday view, by the falsifications to which I have referred.

If the dissected wing of a common bird, such as a pigeon—the left wing—be laid on the table and compared with the plate of *Archæopteryx*, the conclusion that those two wings are essentially alike will be inevitable. It will be impossible to avoid the conclusion that the two digits which support the quills of the ordinary bird existed also in *Archæopteryx*, and their position will be seen to be indicated faintly in the photograph by a shadow which runs parallel with and behind the slender digits. The carpal angle of the wing will be seen in front of the carpal ends of the slender fingers, and from this point the outline of the anterior margin of the wing can be traced to the tip. This margin lies under those fingers. Not only did these, as their form and structure show, *not* support the quills, but they did not even contribute to the support of them. These fingers lie not in the wing at all, but *upon its feather-clad surface.*

Those slender fingers, like the free fingers of the *Pterodactyla*, or of the recent "flying" phalangers and squirrels, or of *Galeopithecus*, or like the pollex of a bat, are admirably adapted for climbing in trees. They proclaim *Archæopteryx* to have been a winged quadruped, and this conclusion receives ample support from the weakness of the vertebral column and of the hind limbs, and from the small size of the pelvis and sacrum.

In the dissected wing, or in any fairly good figure showing both feathers and bones in the wing of an ordinary bird (1, 2), the *ala spuria* will be seen to correspond to one or more of these free fingers of *Archæopteryx*. I cannot at present see any way of deciding whether it is a vestige of one, or of two, or of all three of them, and I cannot satisfy myself as to whether certain slender feathers seen to lie upon the primary quills in the plate, and making a considerable angle with them, are really coverts as usually described, or whether they were attached to the free fingers. If they are coverts, as seems probable, their position (lying *across* the primary quills) may be due to the action of a current of water flowing over the recently dead bird and bringing with it the mud which, being deposited—apparently rather quickly—effected the preservation of the specimen in the perfect state in which it was found.

Dames (3), in his detailed description of the specimen, states that the primary quills ("6 to 7") were attached to the longest finger (II). If while we look at the photograph we consider what would be the result of such an attachment, it must be obvious that it would be twofold. Firstly, the attachment of such a series of quills would render the fingers perfectly useless for climbing, and secondly, a single flap of that wing would twist the phalanges off at the joints. In other words, both wing and finger would be rendered useless by such an attachment.

If Natural Selection has been operating long enough and efficiently enough to determine the evolution of so perfect a series of feathers, it is perfectly certain that that same selection will have led also to the evolution of supports for those feathers as fully fitted to support the feathers as the feathers are fitted for flight; and even if there had been no indication of those supports on the slab, we need have still had no doubt as to their existence.

That the views which have gained such wide acceptance should have been possible when Owen (4) 30 years ago recognised and figured the very bones in question, is a striking testimony to the credulity of modern zoologists!

We read in any text-book that happens to be at hand that the three digits of a bird's wing answer to the "pollex and second and third digits of the pentadactyle fore-limb." Such a statement, though found in almost all modern books which treat of the subject at all, is in no case, so far as I am aware, supported by any evidence whatever. I believe the statement originated in a mistranslation of the

pre-Darwinian statement that the ala spuria is "analogous to the thumb," while the other two digits are called simply "second" and "third," that is *second and third digits* not of the pentadactyle but *of the tridactyle fore limb*. Such phrases in works written on the then undoubted hypotheses of special creation and of fixity of species, could, obviously, not mean that the three digits called "thumb" and "second" and "third" digits had been evolved from the digits I, II, and III of the pentadactyle fore limb of an ancestor: the authors did not believe birds had ever had such ancestors. The transcription of such phrases into post-Darwinian treatises, without consideration of the new meaning which they would thus gain from the new context, appears to have been the origin of the error.

And yet Professor Huxley (5) has relied upon the unfounded statement that these digits are I, II, and III in constructing a theory of the phylogeny of birds. On the strength of this assumption he has denied the descent of birds from Pterodactyles. They may, of course, not be so descended, but we may not reasonably believe that his argument from the wing-structure makes their descent one jot more improbable than it was before.

It may at least be said that, in the matter of pelvis and of wing, *Archæopteryx* is much more like a Pterodactyle than it is like *Compsognathus*, or any other Dinosaur.

Those "ornithic" characters of the Dinosaurs which distinguish them from Pterodactyles, distinguish them also from the undoubted bird *Archæopteryx*; and if the "ornithic" characters of the Dinosaur pelvis generally, and of the foot of *Compsognathus*, are due to any blood-relationship with birds at all, it seems probable that the phylogenetic tree will have to be turned upside down so as to express the descent of Dinosaurs from Carinate birds through a series of "ratite" birds which had lost the power of flight through isolation and consequent suspension of the action of a selection dependent on powers of flight, much as the existing "Ratitæ" have probably descended from Carinates of very various orders.

It is, however, not to put forward new phylogenies that I now write, nor merely to correct a particular error in biological theory, but to point to one of the most fruitful sources of error.

To attempt to expose one by one all the current biological errors would be very much like going to Mecca, armed with a microscope, in order to eradicate cholera by finding the bacilli, and killing them one by one as they were found.

With reference to cholera epidemics, it has been said, "Cholera is a filth-disease: abolish filth, and cholera will vanish."

Uncritical credulity plays the same part in the spread of error as filth plays in the spread of cholera. The standard text-book of the anatomy of the Vertebrata (6) says, "In all Carinatæ there are three digits in the manus, which answer to the pollex, and the second and third digits of the pentadactyle fore limb"; and the statement is not accompanied

by any evidence of any kind in support of it. In a healthy community of students, such statements would do no harm, for only a morbidly-credulous person unfit for scientific work would accept the statement as true, *on the authority* of even the greatest anatomist living. For an honest record of mere fact, we are often compelled to rely upon the accounts given by others—we do not all have the opportunity of dissecting *Nautilus* and other rare animals, and we none of us have time to dissect one-hundredth part of the animals of which specimens are easily obtainable. We are therefore compelled to rely upon each other, and so glaring a falsification as that shown in the Steinmann-Döderlein figure is, fortunately, not common. The discrepancy between the description and the figure (in Zittel, for instance) would, moreover, show any careful reader that either description or figure was false, if not both.

For facts, then, we must rely upon others, but must exercise judgment in doing so.

For opinions it is otherwise. To take another man's opinion and accept it untested—as has been done in the case of the bird's wing—is a sure way to something worse than mere error. Darwin was one of the greatest men who have lived, and his opinion is worth more than that of most other men. To accept even his opinion, however, except after examination of the argument upon which it rests, is evidence that the man so accepting it is unfit for scientific work. He will probably gain applause from "a popular audience" (just as a good encyclopædia may gain approval), but if he holds opinions otherwise than as the result of conviction he is only a walking encyclopædia, and not nearly so good a one as the "Britannica." So long as teachers continue to regard, as many do now regard, the "passing" of their students as the object of their work or of their students' work, so long will the "cramming" system continue to convert men into walking encyclopædias, stocked with second-hand ideas, and incapable of either creating new ideas or of judging of the value of the old ones.

In a paper by Morse (7) the view that the bird's *wing*-digits are II, III, and IV is put forward on the strength of the existence of a supposed vestige of another digit on the radial side. Granting his facts, such a vestige might be a vestige of a pre-pollex, so his contention does not prove even that the usual view is erroneous, though it lessens its apparent probability. It of course does not in the least affect the view I have put forward, for he makes no attempt to prove that the digits *are* II, III, and IV, but only that they are *not* I, II, and III. The possibility of their being III, IV, and V seems not to have occurred to him or to those who have adopted his view. What W. K. Parker (8) regarded as a vestige of the digit IV appears to me to be *os pisiforme*.

Zittel (9) gives a figure described as "nach Owen." Owen's figure shows *four* digits. Zittel has eliminated the innermost. It

would not otherwise fit in with the orthodox view. Owen was a strong opponent of the Dinosaurian-ancestry theory, and his figure was valuable evidence in favour of the Pterodactyle-ancestry view which he maintained. Is it right that Zittel should so falsify that figure as to make it tell the other way, and then describe the mutilated figure as "nach Owen," thus ascribing to Owen a view which he strongly opposed?

The falsification of the figure is a typical example of the inevitable results of "pinning one's faith to a creed," the creed being, in this case, the orthodox view that the three digits in an ordinary bird's wing are I, II, and III.

## SUMMARY.

*Archæopteryx* was a winged quadruped, probably arboreal in habit.

The photograph accompanying this essay shows the primary quills to have been supported by none of the first three digits, and justifies, if it does not even prove, the view that those quills were supported by the digits IV and V (or one of them). Portions of these large digits were figured by Owen thirty years ago, and they are seen in the London specimen and are quite unlike anything seen on the surface of the Berlin specimen, in which these digits probably still lie hidden.

This justifies the belief that the two large digits in an ordinary bird's wing are IV and V, and that the *ala spuria* is a vestige of one or more of the other three digits of the pentadactyle fore limb.

The argument against the view that birds are descended from Pterodactyles is, therefore, worthless so far as it rests upon the assumption that the large digits of the bird's wing are II and III.

The "ornithic" characters of Dinosaurs do not justify the view that birds are descended from these reptiles: for the oldest known bird is devoid of those "ornithic" characters.

*Compsognathus* may form a connecting link (but not, of course, in the direct line) between some unknown "ratite" birds on one hand, and the great Dinosaurs on the other, but only if these be regarded as "degenerate" and overgrown descendants of birds which had lost the power of flight.

Careless transcription of a pre-Darwinian statement (which was perfectly true) into post-Darwinian treatises has given the statement a new and false meaning.

Uncritical plagiarism has rendered the false statement an integral part of almost every modern treatise on the subject.

The false idea expressed in that statement after transcription has dominated the minds of even those (10, 11) who wrote with *Archæopteryx* before their eyes, and the absurdity of a corollary of that idea has induced some (12) to issue a figure so falsified

as to hide the absurdity: and the falsity of that figure has gained for it admission into modern text-books written also under domination of the same false idea, while Owen's figure has been mutilated so as to make it reconcilable with a view which it was intended by Owen to disprove (9).

The spread of error is largely due to the domination of ideas (not invariably false!), and is possible only in a world of uncritical compilers and credulous readers and hearers.

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C. Herbert Hurst.

## V.

# The Problem of Variation.

IT is certain that, in order to get a deeper insight into the causes and modes of organic evolution, we must investigate the problem of variation. The first requisite for such an investigation is a definite grasp of the problem, an analysis of its complications. Much that is written at the present time on ultimate biological theories is more or less barren from the absence of a definite and comprehensive conception of the phenomena which have to be explained. It is easy to expand the proposition that, given all kinds of hereditary variations in individual organisms, selection will accumulate those which lie in a particular direction and result in special adaptations of structure to function. But what causes the variations? It is more important and more difficult to construct a theory of heredity as due to the continuity and conservatism of the germ-plasm, but we have to explain, not merely conservative, but progressive and retrogressive heredity.

It is necessary always to bear in mind that the phenomena to be dealt with are presented by individual organisms. There is no such thing as the origin of species apart from the origin of individuals. It has been maintained by philosophers that a horse can be locked in a stable, but the species horse cannot, being a mental abstraction formed from the perceptions of individuals; but it seems to me more correct to say that we recognise a number of individuals with such a degree of similarity to one another that we class them together and give them all a specific name. It would not be impossible to put all existing horses together into one enclosure.

Variations then, of course, must be variations of individuals; but a most important distinction, which is constantly ignored, exists between contemporary individual differences and the differences between consecutive generations, which form, so to speak, the units of evolution. The nature and degree of individual differences in a single species, the relations of varieties and doubtful species, are important enough as observed facts; but Darwin, Wallace, and others present them as the material on which selection acts, without adequately discussing the question of the relation they bear to the difference between one generation and its successor. Galton and Weldon have recently applied the higher mathematics to the study of individual differences, but it does not seem to me that in this way

we shall make much advance towards an explanation of the variations which make evolution. The differences between a large number of individuals, without reference even to the ontogeny of these differences, are recorded with greater accuracy and completeness than before, but that is all. The same differences might have been present in every generation of the species if it had been created in the form in which we see it.

In order that evolution may take place, individuals must be generated which are different to any that ever existed before. If we take any particular organ or character, one or more individuals in a generation must have possessed this organ or character in a more advanced (or less advanced in the case of retrogression) condition than any individual which had previously existed. This is the kind of variation which we must account for, and find the cause of, in order to have a complete theory of evolution or of heredity. This must occur over and over again to produce the evolution which, according to the evidence, has taken place.

Now the actual appearance of new characters has been observed, for instance, in the case of the Ancon sheep, and in wild plants when cultivated; and, on the other hand, Weismann has proposed the theory that variations of the kind defined above are due to the union of two individuals in sexual reproduction; but I have not space to discuss, on this occasion, either of these two subjects, because, with regard to the second, Weismann has abandoned the theory, and, with regard to the first, such variations occur at the beginning of the development of the individual. Nothing is more certain than that no theory of variation is worthy of attention unless it takes into account the phenomenon of recapitulation. In a very large number of cases an organ, or a character, or an individual, passes through in development stages which reproduce, more or less exactly, an ancestral condition. In other words, new characters are added usually at the end of ontogeny. We have convincing evidence, then, that the modified individual first resembled its parent, and afterwards became different. The ancestor of the flat-fish was symmetrical; but, at some time or other, individuals of this pedigree, after developing into symmetrical fish, became more or less asymmetrical, and every flat-fish *at the present day* develops first into a symmetrical fish, and then turns into a Pleuronectid.

It is certain that no combination of invariable parental germ-plasms could result in this change. How, then, does Weismann's new theory account for it? The determinants, the units of the germ-plasm corresponding to particular cells, or group of cells, in the adult, are modifiable ultimately by external influences. On this new theory Weismann attempts the explanation of recapitulation in the following words:—

"The determinants of the id of germ-plasm become endowed with a greater power of multiplication, so that each one of them causes the

addition of one or more cell-generations to the end of the ontogeny. At the same time, the determinants in the germ-plasm increase in number, and each of them becomes differentiated in a fresh manner. As, however, every two new determinants always follow the same course, from the id of germ-plasm to the final stage in ontogeny as was taken by the single original determinant, they will pass through the same determinant figures as before, and only lead to the formation of new structures in the final stages when they become separated from one another. The ontogenetic stages will be repeated less accurately the nearer development approaches its termination."

Now, for my part, I cannot see that this explanation in any way covers the phenomena. According to the theory, the specialisation of every cell and every organ at every stage depends on determinants. The problem to be explained is, therefore, why the determinants for the organs of the early stages of ontogeny are not modified. Weismann supposes that in the differentiated cells of any stage determinants are used up, while in the undifferentiated multiplying cells unconsumed determinants are held in reserve to supply later generations of differentiated cells. The determinant figure is merely an attempt to show why the consumption of the determinants follows a definite course. It does not afford any reason why the determinants which are reserved to the last should be modified, while those which pass into differentiated cells belonging to early stages remain unaltered.

As to the cause of changes in the determinant, Weismann now attributes them to the direct effect of external influences, which influence the nutrition of the biophors and determinants during their continual reproduction through countless generations; but in his chapter on variation he mentions no reason why such changes should occur rather in the determinants belonging to the final stages of ontogeny, than in those belonging to the earlier stages.

It may be truly said, therefore, that we have no theory of variation which necessarily involves the recapitulation of phylogeny, except the theory of the inheritance of acquired characters. It is important to notice that recapitulations occur chiefly in connection with adaptive modifications. The young flat-fish swims with its principal plane in a vertical position, and its body is symmetrical about that plane; the adult flat-fish, on the contrary, is adapted by its asymmetry to living in a position in which the principal plane of the body is horizontal. The tadpole is essentially a fish adapted to live in water, while the frog is adapted to breathe air and lead a terrestrial existence. These obvious and striking facts point, at all events apparently, to the conclusion that the change of structure was produced by the change of conditions.

Another consideration which points in the same direction is that the actual ontogeny of these changes of structure in the individual has been shown in some instances to be largely dependant on

conditions. For instance, the metamorphosis of tadpoles can be prevented for a long time by certain appropriate conditions, which would not be the case if the changes were entirely determined by modification of the germ-plasm.

A case which I have myself recently investigated experimentally seems to me to support very strongly the theory of the inheritance of acquired characters. I have shown that in normal flat-fishes, if the lower side be artificially exposed to light for a long time, pigmentation is developed on that side; but when the exposure is commenced while the specimens are still in process of metamorphosis, when pigment-cells are still present on the lower side, the action of light does not prevent the disappearance of these pigment-cells. They disappear as in individuals living under normal conditions, but after prolonged exposure pigment-cells reappear. The first fact proves that the disappearance of the pigment-cells from the lower side in the metamorphosis is a hereditary character, and not a change produced in each individual by the withdrawal of the lower side from the action of light. On the other hand, the experiments show that the absence of pigment-cells from the lower side throughout life is due to the fact that light does not act upon that side, for when it is allowed to act, pigment-cells appear. It seems to me the only reasonable conclusion from these facts is that the disappearance of pigment-cells was originally due to the absence of light and that this change has now become hereditary. The pigment-cells produced by the action of light on the lower side are in all respects similar to those normally present on the upper side of the fish. If the disappearance of the pigment-cells were due entirely to a variation of the germ-plasm, no external influence could cause them to reappear, and, on the other hand, if there were no hereditary tendency, the colouration of the lower side of the flat-fish, when exposed, would be rapid and complete.

Weismann admits that hereditary colour-changes, such as a darker colour in butterflies, are produced by climatic influences, but explains this on the supposition that the climatic influences have simultaneously modified in an individual the determinants of the pigmented cells in the integument, and the determinants in the germ-cells of that individual which belong to the corresponding pigmented cells of its offspring. Now it is impossible to suppose that the light acting on one side of a flat-fish would modify the determinants belonging to one side in the ova of that individual, and even if it did it would not cause the pigment-cells to develop symmetrically on both sides of the larva, and then to disappear from one side of the perfect fish.

It is impossible or inconceivable on Weismann's theory that changes of somatic cells should affect the determinants of the germ-cells contained in the soma; there is no connection between the two; but, on the other hand, it is equally inconceivable on his

theory that the presence or absence of the germ-cells in an individual should affect the ontogeny of the soma. He assumes that the determinants for the soma are separated off and determine a certain ontogeny, which is not affected by the fate of the determinants of succeeding generations contained in the germ-cells and germ-tracks; and yet we know that male secondary sexual characters are not developed if the young male is castrated. The development of these characters depends on the normal presence of the testes and their germ-cells in the individual. Weismann mentions this fact, but how does he connect it with his theory? He simply concludes that in such cases the secondary sexual characters of both sexes are present in each individual, and one or the other set develop according to circumstances. He makes no attempt to show why the particular circumstance which determines the development is the presence or removal of the generative organs. The fact of the course of ontogeny in this case being affected in so radical a manner by the mere presence of the generative organs, is alone sufficient to overthrow his fundamental assumption of the essential independence of the soma and the germ-cells contained within it.

Degeneration of organs is a phenomenon in which recapitulation is particularly well exhibited. Scarcely any case is known of an organ which has disappeared or become rudimentary in the adult condition, and which is not more perfectly developed or developed to some extent in earlier stages of the life-history. Even the limbs of snakes are indicated to some extent in the embryo, and the teeth of the right whale are present in the fœtus; but the loss of the eyes in blind subterranean animals is the instance I prefer to bring forward here. On the Natural Selection theory it is no advantage to such animals that the larvæ or the young should have eyes better developed than the adults. Yet this is known to be the case in the blind amphibian Proteus of the caves of Dalmatia, the blind crayfish of the caves of Kentucky, and in other cases. This case has a superiority over others in my argument, because there is no change of conditions as in the frog and the flat-fish: the whole development takes place in the dark. Weismann considers this case in relation to his theory, but does not succeed in showing any logical connection between his assumptions and the phenomenon. He supposes that the determinants for the degenerating organ first begin to lose their power of multiplication, and then a continually increasing number disappear. This means, presumably, that a succession of determinants is necessary to supply the successive generations of cells which keep an organ in proper condition, and that the reserve determinants in the soma are continually multiplying when degeneration does not take place; but we know that in the life of the individual this "multiplication of determinants" is checked or stopped by disuse, and we do not know how the "power of multiplication" is diminished in the germ-plasm. In spite of all that has been discovered or conceived up

to the present, the hypothesis that the properties of the germ-plasm are modified by changes effected in the adult organs by external influences still holds the field.

It must be conceded, even by his opponents, that Weismann has done a very great service to biological science by insisting upon and forcing upon the general attention the actual relations of the germ-cells to the soma. We cannot now overlook the fact that germ-cells are not descended from the modified and specialised cells of the soma, nor can we any longer use a hypothesis of the construction of the germ-cells from gemmules emitted by the somatic cells. The germ-cells, like the cells of a muscle or a gland are derived directly from the ovum by a succession of multiplying cells forming the germ-track; but we have still to discover how the ontogenetic properties of these germ-cells are modified. In the present state of knowledge it is a more irresistible conclusion that changes produced in the soma ultimately affect the germ-cells, than that variations in the latter are due to variations of nutrition in some manner which no one has attempted to describe.

J. T. CUNNINGHAM.

## VI.

# The Nearctic Region and its Mammals.[1]

IT would appear that the Munroe doctrine of "America for the Americans," is little heeded by the biologists of that Continent. Although the best European authorities on the geographical distribution of animals have long ago conceded to the northern half of the New World the rank of one of the six primary divisions of the earth's surface, under the name of the "Nearctic Region," our American friends will have none of it. Two recent writers of deservedly great authority on the Mammal-life of North America, though, as we shall show presently, they differ much in minor details, agree in repudiating the Nearctic Region altogether. They refer the northern parts of North America to the "Arctic Realm" or "Boreal Region," of the Old World, and the southern portion to the "American Tropical Realm" or "Tropical Region," leaving only the intermediate area separate and apart. This intermediate area is included by Mr. Allen in his "North Temperate Realm," which embraces the whole of the northern hemisphere on both sides of the Atlantic, between the annual isotherms of 32° and 70°, but is allowed to rank independently as the "North American Region," while Dr. Merriam, following Cope, calls it the "Sonoran Region." Thus both these authorities agree in splitting up the Nearctic Region into three constituent parts, and in repudiating the views of Sclater and Wallace that it should form one of the main zoo-geographical divisions of the earth's surface. While, however, it must be allowed that both Mr. Allen and Dr. Merriam have studied the distribution of Mammal-life in North America to some effect, and are well acquainted with its details, it will be easy to show that their general views on geographical distribution are not entitled to acceptance. At the same time, we admit that there is something to be said on their side of the question.

Taking a general view of the distribution of Mammal-life over the terrestrial portion of the earth's surface, we see at once that Australia and the adjacent islands stand strongly apart from the rest

[1] i. "The Geographic Distribution of Life in North America, with Special Reference to the Mammalia." By C. Hart Merriam, M.D. *Proc. Biol. Soc. Washington*, vol. vii., p. 1 (April, 1892).

ii. "The Geographical Distribution of North American Mammals." By Joel Asaph Allen. *Bull. Amer. Mus. Nat Hist.*, vol. iv., no. 1 (Dec., 1892).

of the world as being principally tenanted by Marsupials, and as being the sole home of the existing Monotremes. Again, South and Central America together form a division easily separable from the remaining portion of the world, after Australia has been subtracted. The "Neotropical Region," as it is usually called, has a family of Marsupials peculiar to it; it has almost no Insectivores, and it is the home of all the three typical families of the great Order Edentata. Thus we have a very obvious threefold division of the earth's surface, taking Mammals as our text, into what may be called *Notogæa*, *Neogæa*, and *Arctogæa*. The vice of this division is that it leaves the great bulk of the earth's surface (*Arctogæa*), which remains after taking off the Australian and Neotropical Regions, of rather unmanageable size, consisting as it does of North America and the whole of the Old World except Australia and its islands. This large area, however, readily falls into four sections—North America, Europe and Northern Asia, Africa, and Southern Asia, which are denominated by Sclater and Wallace the "Nearctic," "Palæarctic," "Æthiopian," and "Oriental" regions respectively. No one pretends to say that these four regions are exact equivalents in zoological value to the two regions first spoken of—namely, the "Australian" and "Neotropical" regions. As Mr. Wallace ("Geogr. Distr.," vol. i., p. 66) has well put it:—

"It is admitted then that these six regions are by no means of precisely equal rank, and that some of them are far more isolated and better characterised than others; but it is maintained that, looked at from every point of view, they are more equal in rank than any others that can be found; while as regards geographical equality, compactness of area, and facility of definition, they are beyond all comparison better than any others that have yet been proposed for the purpose of facilitating the study of geographical distribution."

Mr. Allen and Dr. Merriam seem to be of opinion that when we come to scientific questions we are bound to throw away considerations of convenience altogether and to stick to matters of fact. This is a beautiful doctrine, but if the matters of fact are in dispute, as is the case in many of the details of geographical distribution, owing to our as yet imperfect knowledge of the subject, it is far better to adopt some easily comprehensible system, of which the leading features are obviously correct, than to use a more elaborate plan, based upon details that are more or less open to question. Contrast, for example, the six divisions of the world already given, their simple names and their easily-defined boundaries, with Mr. Allen's seven "Primary Life-Regions," their complicated titles and their uncertain limits. Where are we to draw the line between the "American Tropical" and the "South American Temperate Realm," or between the "Arctic" and the "North Temperate," both of which embrace portions of the Old and the New Worlds? Than Africa, as regards its Mammals at least, no part of the world, except Australia, has a better set of characteristic types, such as *Hippopotamus*, *Camelopardalis*, *Orycteropus*, *Hyrax*, and many others. Yet Mr. Allen proposes to

unite Africa with India as the "Indo-African Realm," while he would actually split up South America between two different regions. In these proposals, at least, very little consideration is shown to the much-lauded principle of the equivalence in value of the realms or regions.

While, however, we insist upon the propriety of retaining the Nearctic Region as a whole, and as one of the primary zoo-geographical divisions of the earth's surface, we are quite disposed to attend to the views of Messrs. Merriam and Allen as to the best mode of subdividing the large area, and to admire the maps and tables in which they have set it forth. Take, for example, Mr. Allen's first map of his so-called "Realms" of North America (*Bull. A. M. N. H.*, vol., iv., pl. v.). Here we find a threefold division of the continent proposed into "Arctic," "Temperate," and "Tropical" Realms. The "Arctic Realm," which consists merely of the land bordering the Polar Ocean and Hudson's Bay and the great peninsula of Greenland, and is "beyond the limit of arboreal vegetation," Mr. Allen unites to the similar arctic portion of the Old World, stating, no doubt quite correctly, that it is really a part of a "homogeneous hyperborean fauna of circumpolar distribution." But looking to the extreme proverty of life in these inclement latitudes, as Mr. Allen well puts it, we think it quite unnecessary to elevate this wretched fraction of the earth's surface to one of its principal constituent life-regions, and must prefer the plan adopted by Sclater and Wallace, of regarding it as a borderland between the Nearctic and Palæarctic Regions.

We now come to Mr. Allen's "North American Region," which is regarded by the author as a subdivision of the "North Temperate Realm," corresponding in value to what is usually called the "Palæarctic Region" of the Old World, but what Mr. Allen prefers to denominate by the horrible compound term "Eurasia." Mr. Allen's "North American Region" embraces, as will be seen by a glance at the map already referred to, by far the largest portion of that continent. In fact, it embraces the whole, except the extreme arctic portion already referred to, and what Mr. Allen terms "Tropical North America," which consists of the southern end of the peninsula of Florida and a narrow strip of the coast of Mexico on both sides, extending on the Atlantic side up to the Rio Grande and on the Pacific side up to Mazatlan. This "Tropical North America" of Mr. Allen, however, is, in fact, merely the borderland between the Nearctic and Neotropical Regions of Sclater and Wallace, and may be left out of account when a general view of the great life-regions of the world is being taken. We thus see that Mr. Allen's "North American Region" is, for all practical purposes, identical with the Nearctic Region of Sclater and Wallace. Let us now see how Mr. Allen proposes to divide it—according to its Mammal-life.

"The North American Region," Mr. Allen tells us (*op. cit.*, p. 221), "falls into two sub-regions, namely (1) a 'Cold-Temperate Sub-

region' extending southward to about the mean latitude of the Great Lakes, with outlying portions extending further southwards along the principal mountain-system of the continent, and (2) a 'Warm-Temperate Sub-region' occupying the remaining area." This mode of division (which is shown on Mr. Allen's plate vi.) corresponds pretty nearly with that proposed by Dr. Merriam (see his Bio-geographic Map), who calls those two sub-regions "Boreal" and "Sonoran"; but upon comparing the maps of the two authors together, it will be seen at a glance that the minor subdivisions do not exactly correspond, and indeed such particulars must be always more or less matters of opinion.

In his carefully drawn up tables of North American Mammals, Mr. Allen shows that fourteen genera occur in his Cold-Temperate Sub-region, which do not range to any extent into the Warm-Temperate Sub-region. On the other hand, 33 genera found in the Warm-Temperate do not occur in the Cold-Temperate, while 27 genera are, to a greater or less extent, common to both their sub-regions. He points out that the 42 genera of the Cold-Temperate Sub-region are "either obviously of boreal origin, or find their nearest relationship with boreal types," while of the 62 genera which occur in the Warm-Temperate Sub-region, "about fourteen are wide-ranging southern or subcosmopolitan types, 24 may be regarded as indigenous, and about thirteen are of southern origin."

Neglecting, therefore, as we have already proposed to do, Mr. Allen's strip of the "Arctic Realm" in the North American Fauna as merely borderland, and, in a similar way, treating the area south of the Mexican Tableland and the extreme southern parts of the peninsulas of Lower California and Florida as merely debatable land between the Nearctic and Neotropical Regions, we find Mr. Allen's "North American Region of the North Temperate Realm" practically identical with the Nearctic Region of Sclater and Wallace. Following the guidance of Mr. Allen and Dr. Merriam, we recognise two sub-regions only in the region, namely, a Northern and a Southern one.

The Northern Sub-region is composed of Mr. Allen's "Cold-Temperate Sub-region," and the adjacent district on the north which he assigns to the "Arctic Realm." Here the general facies of the Mammal-fauna is much more decidedly similar to that of the Palæarctic Region than in the Southern Sub-region. Such genera as *Cervus*, *Alces*, *Rangifer*, *Ovis*, *Castor*, *Lagomys*, *Gulo*, and *Putorius*, betray at once a very strong Palæarctic element. At the same time, a decidedly endemic element is shown by such types as *Haploceros*, *Fiber*, *Condylura*, *Procyon*, *Mephitis*, and *Taxidea*, which are absolutely unknown in the Palæarctic Mammal-fauna. For this Northern Sub-region of the Nearctic Region perhaps "Canadian" would be the best term, as it embraces the whole of the Dominion of Canada, and the area of the sub-region is therefore at once recognisable by the name. Mr. Allen's term "Boreal" is much too vague.

The Southern Sub-region is composed of Mr. Allen's "Warm Temperate Sub-region," and the fragmentary bits of borderland to the south, which he assigns to the "American Tropical Realm." For this Sub-region "Sonoran," as used by Dr. Merriam, seems to be a good and appropriate term, much preferable to Mr. Allen's "Warm-Temperate," which is quite indefinite. The Sonoran Sub-region of the Nearctic Region, as we propose to call it, contains a much smaller Palæarctic element than the Canadian Sub-region. Nevertheless, *Bison*, *Tamias*, *Spermophilus*, *Arvicola*, and others, are undoubtedly Palæarctic types. On the other hand, *Antilocapra*, *Cynomys*, *Sigmodon*, *Oryzomys*, *Neotoma*, *Thomomys*, *Dipodomys*, *Scalops*, *Urotrichus*, and *Bassaris*, with their allies, constitute a strong endemic force. But what principally differentiates the Sonoran from the Canadian Sub-region is the presence in it of a lot of intruders from the Neotropical Region—such as *Didelphys*, *Dicotyles*, *Cariacus*, *Tatusia*, *Nasua*, and *Molossus*, which have nothing to do with the autochthonous fauna of North America. On the whole, the Nearctic Mammal-fauna may be defined as having a strong Palæarctic basis mixed up with endemic elements, and invaded largely on its southern frontiers by Neotropical immigrants. The presence of these Neotropical immigrants serves chiefly to distinguish the Sonoran from the Canadian Sub-region, though, as Mr. Allen has shown, there is also a material difference in the endemic forms peculiar to the respective Sub-regions.

## VII.

# The British Association Addresses, 1893.

SUCH authoritative expressions of opinion on current questions of scientific interest as always characterise the Presidential Addresses delivered to the British Association and its several Sections, are of importance in indicating the tendency of research; but it is not often that these general discourses contain much new matter. We are, therefore, not expressing any feeling of disappointment, when we remark upon the conspicuous lack of new facts and unfamiliar ideas in the addresses relating to Natural Science delivered last month at Nottingham. Everyone interested in Physiology, Petrology, Field Natural History, Arctic Exploration, and Anthropology, will have read the carefully-prepared digests of current thought and research with profit; and if there is little in them that is absolutely new, there are at least some personal expressions of opinion of the deepest significance.

Dr. Burdon Sanderson, the President, dealt with the present aspect of the problems of Physiology; and the whole address, full of the history of progress in the science, seems to have been designed to lead up to the climax—a plea for the establishment of a "British Institute of Preventive Medicine."

"It is possible that many members of the Association are not aware of the unfavourable—I will not say discreditable—position that this country at present occupies in relation to the scientific study of this great subject—the causes and mode of prevention of infectious diseases. As regards administrative efficiency in matters relating to public health, England was at one time far ahead of all other countries, and still retains its superiority; but as regards scientific knowledge we are, in this subject as in others, content to borrow from our neighbours. Those who desire either to learn the methods of research or to carry out scientific inquiries, have to go to Berlin, to Munich, to Breslau, or to the Pasteur Institute in Paris, to obtain what England ought long ago to have provided. For to us, from the spread of our race all over the world, the prevention of acute infectious diseases is more important than to any other nation. May I express the hope that the effort which is now

being made to establish in England an Institution for this purpose not inferior in efficiency to those of other countries, may have the sympathy of all present ? "

### Physiology not Physics.

The final expression of Dr. Sanderson's opinion of the nature of physiological processes is of great interest. There is a certain "specific energy of cells" that neither physics nor chemistry can explain ; and physiology can never become a mere branch of applied physics or chemistry. Nevertheless, there are parts of physiology wherein the principles of these sciences may be applied directly. " Thus, in the beginning of the century, Young applied his investigations as to the movements of liquids in a system of elastic tubes, directly to the phenomena of the circulation ; and a century before Borelli successfully examined the mechanism of locomotion and the action of muscles, without reference to any excepting mechanical principles. Similarly, the foundation of our present knowledge of the process of nutrition was laid in the researches of Bidder and Schmidt, in 1851, by determinations of the weight and composition of the body, the daily gain of weight by food or oxygen, the daily loss by the respiratory and other discharges, all of which could be accomplished by chemical means. But in by far the greater number of physiological investigations, both methods (the physical or chemical and the physiological) must be brought to bear on the same question —to co-operate for the elucidation of the same problem. In the researches, for example, which during several years have occupied Professor Bohr, of Copenhagen, relating to the exchange of gases in respiration, he has shown that factors purely physical—namely, the partial pressures of oxygen and carbon dioxide in the blood which flows through the pulmonary capillaries—are, so to speak, interfered with in their action by the ' specific energy ' of the pulmonary tissue, in such a way as to render this fundamental process, which, since Lavoisier, has justly been regarded as one of the most important in physiology, much more complicated than we for a long time supposed it to be. In like manner Heidenhain has proved that the process of lymphatic absorption, which before we regarded as dependent on purely mechanical causes—*i.e.*, differences of pressure—is in great measure due to the specific energy of cells, and that in various processes of secretion the principal part is not, as we were inclined not many years ago to believe, attributable to liquid diffusion, but to the same agency. I wish that there had been time to have told you something of the discoveries which have been made in this particular field by Mr. Langley, who has made the subject of ' specific energy ' of secreting-cells his own. It is in investigations of this kind, of which any number of examples could be given, in which vital reactions mix themselves up with physical and chemical ones so intimately that it is difficult to draw the line between them, that the

physiologist derives most aid from whatever chemical and physical training he may be fortunate enough to possess."

### Uniformitarianism in Geology.

Mr. Teall's address to the Geological Section is the most remarkable for startling expressions of opinion, and may be regarded as indicating another reaction in the ever vacillating body of enquirers into the history of the earth. Speaking as a petrologist, Mr. Teall remarks that although enormous progress has been made in this science during the last hundred years, there has been comparatively little advance so far as broad, general theories relating to the origin of rocks are concerned. He even declares his belief, that those who have deserted the old school of Uniformitarians for a certain modern creed of Evolution, will ere long discover their mistake. "The uniformitarian hypothesis, as applied to the rocks we can examine, has assimilated and co-ordinated so many facts in the past, and is assimilating and co-ordinating so many new discoveries, that we should continue to follow it, rather than plunge into the trackless waste of cosmogonical speculation in pursuit of what may after all prove to be a will-o'-the-wisp." " . . . The good old British ship, 'Uniformity,' built by Hutton, and refitted by Lyell, has won so many glorious victories in the past, and appears still to be in such excellent fighting trim, that I see no reason why she should haul down her colours either to Catastrophe or Evolution. Instead, therefore, of acceding to the request to 'hurry up,' we make a demand for more time. The early stages of the planet's history may form a legitimate subject for the speculations of mathematical physicists, but there seems good reason to believe that they lie beyond the ken of those geologists who concern themselves only with the record of the rocks."

### Crystalline Schists.

The earliest sedimentary rocks in Britain, Mr. Teall admits, present some differences from the later rocks in this country, but he doubts whether any conclusions of universal application can be drawn from the fact. The common idea, however, that crystalline schists belong exclusively to the earliest periods of the earth's history, and therefore are witnesses of cosmic evolution, is combated in a vigorous manner. "The crystalline schists certainly do not form a natural group. Some are undoubtedly plutonic igneous rocks showing original fluxion; others are igneous rocks which have been deformed by earth-stresses subsequent to consolidation; others, again, are sedimentary rocks metamorphosed by dynamic and thermal agencies, and more or less injected with 'molten mineral matter'; and lastly, some cannot be classified with certainty under any of these heads. So much being granted, it is obvious that we must deal with this petrographical complex by separating from it those rocks about the origin of which

there can be no reasonable doubt. Until this separation has been effected, it is quite impossible to discuss with profit the question as to whether any portions of the primitive crust remain. In order to carry out this work it is necessary to establish some criterion by which the rocks of igneous may be separated from those of sedimentary origin. Such a criterion may, I think, be found, at any rate in many cases, by combining chemical with field evidence. If associated rocks possess the composition of grits, sandstones, shales, and limestones, and contain also traces of stratification, it seems perfectly justifiable to conclude that they must have been originally formed by processes of denudation and deposition. That we have such rocks in the Alps and in the Central Highlands of Scotland, to mention only two localities, will be admitted by all who are familiar with those regions. Again, if the associated rocks possess the composition of igneous products, it seems equally reasonable to conclude that they are of igneous origin. Such a series we find in the North-West of Scotland, in the Malvern Hills, and at the Lizard. In applying the test of chemical composition, it is very necessary to remember that it must be based, not on a comparison of individual specimens, but of groups of specimens. A granite and an arkose, a granitic gneiss and a gneiss formed by the metamorphosis of a grit, may agree in chemical and even in mineralogical composition. The chemical test would therefore utterly fail if employed for the purpose of discriminating between these rocks. But when we introduce the principle of paragenesis it enables us in many cases to distinguish between them. The granitic gneiss will be associated with rocks having the composition of diorites, gabbros, and peridotites; the sedimentary gneiss with rocks answering to sandstones, shales, and limestones. Apply this test to the gneisses of Scotland, and I believe it will be found in many cases to furnish a solution of the problem."

" . . . The origin of gneisses and schists, in my opinion, is to be sought for in a combination of the thermal and dynamic agencies which may be reasonably supposed to operate in the deeper zones of the earth's crust. If this view be correct, it is not improbable that we may have crystalline schists and gneisses of post-Silurian age in the North-West of Europe formed during the Caledonian folding, others in Central Europe of post-Devonian age due to the Hercynian folding, and yet others in Southern Europe of post-Cretaceous age produced in connection with the Alpine folding. But if the existence of such schists should ultimately be established, it will still probably remain true that rocks of this character are in most cases of pre-Cambrian age. May not this be due to the fact, suggested by a consideration of the biological evidence, that the time covered by our fossiliferous records is but a small fraction of that during which the present physical conditions have remained practically constant?"

### The Polar Basin.

The address of Mr. Seebohm to the Geographical Section is perhaps the most readable, but it contains nothing new beyond six beautiful maps, illustrating the River Basins, Temperature, Rain and Snow, Heights and Depths, and Vegetation, prepared under the direction of Mr. E. G. Ravenstein. The general reader, however, will perhaps obtain a clearer broad view of the Arctic Regions from this carefully-prepared address, than from any previously-published description.

### Zoo-geographical Regions.

Incidentally Mr. Seebohm expresses his views on the possibility of dividing the earth's surface into regions, and then makes some interesting remarks on the special case of the Polar Basin. "The fact is that life areas, or zoo-geographical regions, are more or less fanciful generalisations. The geographical distribution of animals, and probably also that of plants, is almost entirely dependent upon two factors, climate and isolation, the one playing quite as important a part as the other. The climate varies in respect of rainfall and temperature, and species are isolated from each other by seas and mountain ranges. The geographical facts which govern the zoological provinces consequently range themselves under these four heads. It is at once obvious that the influences which determine the geographical distribution of fishes must be quite different from those which determine the distribution of mammals, since the geographical features which isolate the species in the one case, are totally different from those which form impassable barriers in the other. It is equally obvious that the climatic conditions which influence the geographical range of mammals, must include the winter cold as well as the summer heat, while those which determine the geographical distribution of birds, most of which are migratory in the Arctic Regions, is entirely independent of any amount of cold which may descend upon their breeding grounds during the months which they spend in their tropic or sub-tropic winter quarters. Hence all attempts to divide the Polar Basin into zoological regions or provinces are futile. Nearly every group of animals has zoological regions of its own, determined by geographical features peculiar to itself, and any generalisations from these different regions can be little more than a curiosity of science. The mean temperature or distribution of heat can be easily ascertained. It is easy to generalise so as to arrive at an average between the summer heat and the winter cold, because they can be both expressed in the same terms. When, however, we seek to generalise upon the distribution of animal or vegetable life, how is it possible to arrive at a mean geographical distribution of animals? How many genera of molluscs are equal to a genus of mammals, or how many butterflies are equal to a bird? If there be any region of the world with any claim to be a life area, it is that part of the Polar

Basin which lies between the July isotherm of 50 deg. or 53 deg. F. and the northern limit of organic life. The former corresponds very nearly with the northern limit of forest growth, and they comprise between them the barren grounds of America and the tundras of Arctic Europe and Siberia. The fauna and flora of this circumpolar belt is practically homogeneous; many species of both plants and animals range throughout its whole extent. It constitutes a circumpolar Arctic region, and cannot consistently be separated at Behring Strait into two parts of sufficient importance to rank even as sub-regions. The mean temperature of a province is a matter of indifference to some plants and to most animals. The facts which govern their distribution are various, and vary according to the needs of the plant or animal concerned. To a migratory bird the mean annual temperature is a matter of supreme indifference. To a resident bird the question is equally beside the mark. The facts which govern the geographical distribution of birds are the extremes of temperature, not the means. Arctic birds are nearly all migratory. Their distribution during the breeding season depends primarily on the temperature of July, which must range between 53 deg. and 35 deg. F. It is very important, however, to remember that it is actual temperature that governs them, not isotherms corrected to sea-level. If an Arctic bird can find a correct isotherm below the Arctic circle by ascending to an elevation of 5,000 or 6,000 ft. above the level of the sea, it avails itself of the opportunity. Then the region of the Dovrefield above the limit of forest growth is the breeding place of many absolutely Arctic birds; but this is not nearly so much the case on the Alps, because the cold nights vary too much from the hot days to come within the range of the birds' breeding grounds. Here, again, the mean daily temperature is of no importance. It is the extreme of cold which is the most potent factor in this case, and no extreme of heat can counterbalance its effect."

### The Field Naturalist.

Canon Tristram's address to the Biological Section, as might be expected, is devoted to the interests of the Field Naturalist. It is a matter of gratification, for once, to turn from the modern professor and his laboratory to a distinguished biologist whose work has been chiefly accomplished in the field. Observations of organisms in their natural surroundings are not merely essential for the understanding of such broad questions as geographical distribution, variation, mimicry, migration, and so forth; but they are also, as the Canon points out, often indispensable even in researches for which museum cabinets are commonly deemed to furnish adequate material.

"The closet systematist is very apt to overlook or to take no account of habits, voice, modification, and other features of life which have an important bearing on the modification of species. To take one instance, the short-toed lark (*Calandrella brachydactyla*) is spread

over the countries bordering on the Mediterranean; but, along with it, in Andalusia alone is found another species, *Cal. bætica*, of a rather darker colour, and with the secondaries generally somewhat shorter. Without further knowledge than that obtained from a comparison of skins, it might be put down as an accidental variety. But the field naturalist soon recognises it as a most distinct species. It has a different voice, a differently shaped nest; and, while the common species breeds in the plains, this one always resorts to the hills. The Spanish shepherds on the spot recognise their distinctness, and have a name for each species. Take, again, the eastern form of the common song-thrush. The bird of North China, *Turdus auritus*, closely resembles our familiar species, but is slightly larger, and there is a minute difference in the wing formula. But the field naturalist has ascertained that it lays eggs like those of the missel-thrush, and it is the only species closely allied to our bird which does not lay eggs of a blue ground colour. The hedge accentor of Japan (*Accentor rubidus*) is distinguished from our most familiar friend, *Accentor modularis*, by delicate differences of hue. But, though in gait and manner it closely resembles it, I was surprised to find the Japanese bird strikingly distinct in habits and life, being found only in forest and brushwood several thousand feet above the sea. I met with it first at Chinsenze—6,000 ft.—before the snow had left the ground, and in summer it goes higher still, but never descends to the cultivated land. If both species are derived, as seems probable, from *Accentor immaculatus* of the Himalayas, then the contrast in habits is easily explained. The lofty mountain ranges of Japan have enabled the settlers there to retain their original habits, for which our humbler elevations have afforded no scope."

### Migration of Animals.

In the long-studied subject of the migration of birds and other animals, the field naturalist can also still find continuous and profitable occupation; and Canon Tristram calls attention especially to an exceptional phenomenon, "Not the mere wanderings of waifs and strays, nor yet the uncertain travels of some species, as the crossbill in search of food, but the colonising parties of many gregarious species, which generally, so far as we know in our own hemisphere, travel from east to west, or from south-east to north-west. Such are the waxwing (*Ampelis garrula*), the pastor starling (*Pastor roseus*), and Pallas's sandgrouse, after intervals sometimes of many years, or sometimes for two or three years in succession. The waxwing will overspread Western Europe in winter for a short time. It appears to be equally inconstant in its choice of summer quarters, as was shown by J. Wolley in Lapland. The rose pastor regularly winters in India, but never remains to breed. For this purpose the whole race seems to collect and travel north-west, but rarely, or after intervals of many years, returns to the same

quarters. Verona, Broussa, Smyrna, Odessa, the Dobrudscha, have all during the last half-century been visited for one summer by tens of thousands, who are attracted by the visitations of locusts, on which they feed, rear their young, and go. These irruptions, however, cannot be classed under the laws of ordinary migration. Not less inexplicable are such migrations as those of the African darter, which, though never yet observed to the north of the African lakes, contrives to pass, every spring, unobserved to the lake of Antioch in North Syria, where I found a large colony rearing their young, which, so soon as their progeny was able to fly, disappeared to the south-east as suddenly as they had arrived."

### The Perfection of the Human Frame.

The last of the addresses requiring notice, that of Dr. Munro to the Anthropological Section, will scarcely bear abstracting. It discusses, in an interesting manner, the direct and collateral advantages conferred on man by the erect position of his frame; and there are some noteworthy remarks on the theory of Natural Selection as applied to man. Dr. Munro asks Dr. A. R. Wallace to explain why, in his philosophy, he dispenses with the operation of a "higher intelligence" in the early stages of man's evolution, and finds its assistance only requisite to give the final touches to humanity. We shall be interested to learn the reply.

# SOME NEW BOOKS.

NATURALIST'S MAP OF SCOTLAND. By J. A. Harvie-Brown and J. G. Bartholomew. Edinburgh: John Bartholomew & Co., 1893. Price 2s. 6d.

THIS is a beautifully-executed map of Scotland, on the scale of 10 miles to an inch, full of information of value not only to the naturalist, but also to tourists and sportsmen. The various "Faunal Areas" are regarded as coincident with the river basins, and the lines of demarcation are accordingly marked along the watersheds. The areas of cultivated land, patches of woodland, bare moorland and other uncultivated land, with the deer forests, are all indicated by different colours. The areas above 1,000 and 2,000 ft. in elevation respectively are marked by special shading; and the depth of the sea is shown by various tints of blue. As the authors remark, much of their work is tentative and liable to modification by future research; but no stronger incentive to this important research could have been devised than the result of their painstaking labours now before us.

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A CONTRIBUTION TO THE GEOLOGY AND NATURAL HISTORY OF NOTTINGHAMSHIRE. Edited by J. W. Carr, M.A., F.G.S. Small 8vo. Pp. 90. Nottingham: James Bell, 1893. Price 2s. net.

THIS little work was specially prepared for the use of members of the British Association during the recent meeting, and is a valuable compendium of the Geology, Zoology, and Botany of the district of which it treats. Contributions to the geological section are made by Messrs. J. Shipman and R. M. Deeley, and the Rev. J. M. Mello; the notes on birds were written by Mr. F. B. Whitlock, and the list of mollusca was compiled by Mr. B. Sturges Dodd. The list and notes on the vascular plants are contributed by Mr. H. Fisher. Nottinghamshire is a rather monotonous county of cultivated land, almost entirely within the drainage area of the Trent; and the greater part of its surface is occupied by the New Red Sandstone. The fauna and flora, however, is far from meagre, and much still remains to be done in recording the distribution of many groups both of animals and plants.

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A BIOGRAPHICAL INDEX OF BRITISH AND IRISH BOTANISTS. By J. Britten and G. S. Boulger. 8vo. Pp. xvi., 188. London: West, Newman & Co., 1893. Price 5s.

WE are glad to welcome the re-issue, in one volume, of Messrs. Britten and Boulger's Index of Botanical Biography. It has been promised for some time, but the authors assure us that the amount of corrections and amplifications necessary have justified the delay. The

record has been carried down to the end of 1892, and the extent of the revision "may be gauged from the fact that, whereas the issue in the *Journal of Botany* comprised 1,619 names, occupying 148 pages, an average of over ten names to the page, in its present form our little book contains 1,825 names, and occupies 188 pages, an average of less than ten names to the page." Even had there been no additions we should have commended a re-issue, for a bibliography scattered through the pages of three volumes of the *Journal of Botany* compares unfavourably with a compact little volume like that now before us. As the title implies, the work is an index, "intended mainly as a guide to further information," and as such is admirable; references to the chief sources of further information being freely given.

The term botanist is held to include all who have in any way contributed to the literature of botany, who have made scientific collections of plants, or have otherwise assisted directly in the progress of botany, exclusive of pure horticulture. Mere patrons have, as a rule, not been included, or those known only as contributing small details to a local flora. The authors have done their work well and made a valuable contribution to botanical literature. Bibliography is an endless task and, doubtless, one might by toil find additions even to the long roll of 1,825 names, among which, by the way, we do not find that of Gilbert White, of Selborne, who surely might claim to be inserted equally with some of those whose names are included.

The book is nicely printed, and remarkably free from typographical errors.

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A Descriptive Account of the Mammals of Borneo. By Charles Hose, F.R.G.S., F.Z.S. Pp. 78, with map and three plates. Diss, Norfolk: Edward Abbot, 1893.

The excellent exploring work which Mr. Charles Hose, one of the Residents in the service of the Rajah of Sarawak, has been doing in Borneo has been for some time known to most English scientific men, partly through the receipt by many different museums of zoological specimens of his collecting, and partly by the constantly-recurring descriptions in scientific periodicals of new vertebrates and invertebrates discovered by him in that wonderful island, the riches of which still seem so far from being exhausted.

During a recent visit to Europe, Mr. Hose has found time to compile, originally merely for his own and his friends' use in the jungle, the descriptions of all the mammals known to occur in the island; and at the request of some of his scientific friends he has added to the rough notes thus made some short original remarks drawn from his own knowledge of the species. This being the origin of the unpretentious little book above quoted, any detailed criticism of it would be out of place, and it should be accepted simply as the first rough basis on which, as we may hope, the author will found such a complete work as may be worthy of his intimate knowledge of the Bornean wild beasts and their ways.

The descriptions in this book are adapted almost or quite *verbatim* from the technical writings of recent workers on the subject, and mainly from Blanford's "Mammals of India," and Anderson's "Zoology of the Yunnan Expedition"; those of the species more recently discovered being, of course, taken from the original notices. Following the descriptions, Mr. Hose has given his own notes, which we hope will, in the future, be largely amplified, on the habits,

comparative rarity, ranges, native names, and other such particulars of the animals as can only be learnt on the spot, and then only by an enthusiastic lover of the subject.

In all 144 species are recognised as inhabitants of Borneo, and of these about twenty of all sorts, from monkeys to shrews and mice, have been added to the fauna by Mr. Hose and his friend and co-worker, Thos. A. H. Everett, to whose advice and encouragement zoological science is largely indebted for the enrolment of such an ardent recruit as our author has proved himself to be.

Such work as Mr. Hose's is a type of what ought to be going on all over the world, wherever Englishmen with their national love of sport and natural history are living in the midst of a fauna as yet undisturbed by the onward march of civilisation. As European settlements and colonies gradually cover the earth the native fauna is inevitably killed out, either directly for sport or food, or indirectly by the introduction of domestic and parasitic animals. Now, and for the next twenty years, is, therefore, the time that our museums should be filled with carefully-preserved specimens of all sorts, so that our successors may have some chance of actually seeing examples of the animals which will soon no longer exist in nature. There can be no greater absurdity than the recent clamour made by certain "nature-lovers" in decrying the killing of specimens for collections, since such collecting can have not the slightest appreciable effect in exterminating species in comparison with many other processes now going on, notably the barbarous slaughter of birds for their feathers; a method by means of which millions of individuals are killed and their spoils thrown away after a year or two's administering to someone's vanity and thoughtlessness. How great is the destruction thus worked is to be gathered from the fact that the British Museum collection of birds, the largest in the world, does not consist of so many individual skins as have been sold for the purposes of fashion at a single day's sale at a City warehouse. But, protest as we may, the sad fact has to be faced that many of the most interesting and beautiful species now existing are doomed to destruction, and the liberal storage of specimens in museums for careful and long preservation appears to be the only means to give our successors the advantage we ourselves enjoy; and to decry such accumulations is a form of sentimental selfishness of which we hope no reader of NATURAL SCIENCE will be guilty. Such work as Mr. Hose's, therefore, combining, as it does, both observation and collection, is worthy of the highest commendation, and we may well wish him success in his future studies of the rich Bornean fauna.

O. T.

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THE AMPHIOXUS AND ITS DEVELOPMENT. By Dr. B. Hatschek. Translated and Edited by James Tuckey, M.A., Lecturer in the University of Durham. Pp. 181, with nine full page illustrations. London: Swan Sonnenschein & Co., 1893. Price 6s.

IT is difficult to know exactly what to say about this book. It is a bald translation of Dr. Hatschek's researches on Amphioxus with marvellously incapable reproductions of the beautiful figures. The excuse of the "translator and editor" for the book is that hitherto "these investigations have not been accessible to that portion of the scientific world which does not read German." It may be laid down dogmatically that that portion of the scientific world which does not read German has no business with original memoirs. The results of

memoirs are collated and simplified for them in a multitude of excellent text-books, and the perusal of translations of isolated researches can but bloat them up with unassimilated materials, and give them a lopsided view of the animals they wish to study. Take this particular case. Dr. Hatschek has written more and discovered more about Amphioxus than any other investigator; but many others have written on it, and our total knowledge of Amphioxus is based on a very large number of original papers. About these others the "editor" says no word, and anyone judging by this book would conclude that he had exhausted the subject, whereas by a little laboratory work and a perusal of "Marshall and Hurst" or Parker's "Elementary Biology" he would have learned much more. The most astonishing thing about this publication is that it forms one of a series of "Introductory Science Text-Books," for original papers come at the end, not at the beginning of biological work.

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Report on the European Methods of Oyster-culture. By Bashford Dean. Bull. U.S. Fish Commission for 1891. Pp. 357-406, pls. lxxv.-lxxxviii. 1893.

This report is drawn up on the author's personal observations made during a tour through Europe for the special purpose in 1891. Previous reports by Dr. Dean have been noticed in this Journal for Feb., 1893. Detailed descriptions of oyster-culture as carried on in Italy, Spain, Portugal, Germany, Holland, Belgium, and England are given, that of France forming the subject of one of the previous reports. Dr. Dean gives an account of the actual processes of oyster-culture, shows in a general way the influence exerted upon the industry by Government concessions or restrictions, and deals with a variety of questions relating to the living conditions of the oyster. The report is full of useful and practical remarks, is illustrated by photographs taken by the author, and we can only repeat what we said on the previous occasion, that it should be furnished to every grower around our coasts.

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Ethnographie Nordost-Afrikas: Die materielle Cultur der Danâkil, Galla und Somâl. By Philipp Paulitschke. 8vo. Pp. 338, with 25 plates containing 100 figures and map. Berlin: 1893. Price 20 marks.

This volume commences with a geographical sketch of the district, notes on the peoples, the pure and mixed races, and then deals with their culture. Clothing, ornament, weapons, building construction, utensils, foods, physiology and hygiene, family and social life, are all taken in turn and fully dealt with; these are followed by chapters on economics, such as production and sale of valuables, imports, value of the natives and their work. The plates represent the natives themselves and their industries; and the map, on the scale of 1 : 4,000,000, shows the distribution of the various races.

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Guide to the Siebengebirge. [Führer durch das Siebengebirge.] By B. Stürtz. Bonn: A. Henry, 1893.

Naturalists visiting the Rhine will welcome this valuable little guide-book as affording not merely the usual information for tourists, but also a concise and interesting notice of the Geology, Zoology,

and Botany of the region immediately to the south of Bonn. It is written in popular style by Mr. Stürtz, the well-known dealer in geological specimens at Bonn, and is accompanied by a frontispiece giving a general view of the Siebengebirge. There are also lists of maps and works of reference.

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A Classed and Annotated Bibliography of the Palæozoic Crustacea, 1698–1892, to which is added a Catalogue of North American Species. By Anthony W. Vogdes. Occasional Papers IV., California Academy of Science. 8vo. Pp. 416. San Francisco: June, 1893. Price 10s.

This valuable book, as its title indicates, is divided into parts. The bibliography (part 1) occupies 252 pages, and is followed by a catalogue of Trilobites (part 2), arranged firstly under the families, and secondly in alphabetical order of genera. The third part (pp. 361–414) treats of the non-trilobitic genera and species, which follow precisely the same convenient arrangement as do the Trilobites. The volume appears to be compiled in the author's usual careful manner.

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The Clarendon Press announces the issue of part i. of Mr. B. D. Jackson's "Index Kewensis," a monumental index to the names (with authorities) of all known flowering plants, with an indication of their geographical distribution. The part comprises 728 pp., 4to, and is issued to subscribers at two guineas net. Sir Joseph Hooker, who has supervised the work, thus explains its origin:—"Shortly before his death, Mr. Darwin informed me of his intention to devote a considerable sum in aid or furtherance of some work of utility to biological science; and to provide for its completion, should this not be accomplished during his lifetime. He further informed me that the difficulties he had experienced in accurately designating the many plants which he had studied, and ascertaining their native countries, had suggested to him the compilation of an Index to the Names and Authorities of all known Flowering Plants and their Countries, as a work of supreme importance to Students of Systematic and Geographical Botany, and to Horticulturists, and as a fitting object of the fufilment of his intentions. I have only to add that, at his request, I undertook to direct and supervise such a work; and that it is being carried out at the Herbarium of the Royal Gardens, Kew, with the aid of the staff of that establishment."

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The third part of the new edition of the Molluscan portion of "Dr. H. G. Bronn's Klassen und Ordnungen des Thier-Reichs" is just out, and permits one to judge of its merits. So far the work is in every way a worthy successor to the previous edition, and the fact that it is being brought out under the able editorship of Dr. Simroth, of Leipsic, is a guarantee that this standard of excellence will be maintained to the end.

The first two lieferungen, issued last year, were devoted to an historical summary; the new part, which ranks as lief. 3 to 6, contains a general bibliography and the beginning of the Aplacophora, with the first four plates.

By some curious oversight, while many of the minor papers on

molluscan adults are duly noted in the bibliography, the standard work by Troschel, "Das Gebiss der Schnecken," has been omitted.

The treatment of the subject matter of the work, as evinced by the portion before us, leaves little, if anything, to be desired, and the plates are most excellent. It is to be hoped that the work will not take as long to complete as some other Teutonic productions that could be named.

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Mr. H. A. Pilsbry has completed and issued the last part of vol. xiv. of Tryon's "Manual of Conchology." The volume is illustrated with 68 plates, and is prefaced by a general account of the structure and classification of the Chitons. An index to these molluscs will appear with the next part, which completes their description.

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The last part (fasc. v.) of Professor A. de Lapparent's "Traité de Géologie" (pp. 1281 to end) has been issued by F. Savy, Paris. Messrs. Macmillan & Co. announce the preparation of a third edition of Sir Archibald Geikie's well-known Text-book.

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Messrs. W. H. Allen & Co. will shortly publish a new edition of "Our Reptiles and Batrachians," by Mr. M. C. Cooke, which has long been out of print. The work has been revised and corrected to date by the author with some additional matter. The coloured plates are reproduced in chromolithography.

The same publishers have also nearly ready for publication a "Handbook of British Hepaticæ," by Mr. Cooke, with introduction and full description of all the genera and species hitherto found in the British Isles, illustrated by 200 woodcuts and seven plates. The work will be the only complete guide to the subjects which has been published in this country for upwards of a quarter of a century.

# OBITUARY.

## EDUARD SCHNITZER.

BORN MARCH 28, 1840. DIED DECEMBER, 1892.

NEWS has been received during the past month of the sad murder of Dr. Eduard Schnitzer, better known as Emin Pasha. The event took place at the close of last year.

Eduard Schnitzer was born on the 28th of March, 1840, in Oppeln, in the Prussian province of Silesia. He was the son of Ludwig Schnitzer and Pauline his wife. The family removed to Neisse in 1842. After being educated in the Gymnasium of Neisse, Eduard commenced the study of medicine at Breslau in 1858. He completed his studies in Berlin, where he attended the University during 1863–1864 and graduated.

A strong desire for travel led the young doctor to look for a sphere of work in a foreign land, and leaving Berlin at the end of 1864, he went to seek a practice in Turkey. Chance led him to Antivari and Scutari, where he obtained the confidence of the Vali Mushir Divitji Ismail Hakki Pasha, from whom he received a post on his staff, and whom he accompanied on his official journeys throughout the various provinces of the extensive district under his jurisdiction. In this way Schnitzer became acquainted with Armenia, Syria, and Arabia, and at length Constantinople, where Hakki Pasha died in 1873.

In 1875, Schnitzer returned to Neisse, devoting his leisure to Natural History. Suddenly he started off for Egypt, and in 1876 we find him entering the Egyptian service as Dr. Emin Effendi. He was ordered to join the Governor-General of the Soudan at Khartoum, and from there was sent to act as chief medical officer in the Equatorial Provinces, of which Gordon was the Governor.

In 1878, on Gordon accepting the Governor-Generalship of the Soudan, he appointed Emin Governor of the Equatorial Provinces. Immediately setting to work to restore matters, his success was unbounded, and by 1879, Felkin says, a most wonderful change had taken place. "Stations had been rebuilt, discontent was changed to loyal obedience, corruption had been put down, taxation was equalised, and he had already begun the task of clearing his province from the slave-traders who infested it." All this, with the exception of a few months' help from Lupton

Bey, he did entirely alone and unassisted. He received the title of Bey in 1879, and by the end of 1882 he was able to report that not only was his province at peace and contented, but that he had entirely banished the slave-traders from his borders. He was also able to show a profit of £8,000 in that year, whereas when he took up the work there was a deficit of £32,000 per annum.

The reader is referred to Felkin's life of Emin, which forms the Introduction to "Emin Pasha in Central Africa," London, 1888, from which much of our information is taken.

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## EDWARD CHARLESWORTH.

A REMARKABLE man in many respects was Mr. Charlesworth, whose death, at an advanced age, took place recently at Saffron Walden. Educated at Guy's Hospital, he gained a good general knowledge of Comparative Anatomy, and while still a student, he came prominently into notice by the publication in 1835 of a masterly paper on the "Crags of East Anglia." He then pointed out that the Crag of Suffolk was divisible into two portions, termed respectively the Coralline and the Red Crags. Those divisions were accepted by Lyell, and their names have become permanently established. Later on he pointed out that in Norfolk a newer division occurred, and this he named the Mammaliferous Crag, now generally known as the Norwich Crag.

In 1836, Charlesworth was temporarily employed in the British Museum, but, having busied himself with the invention of an "elevator gun," he soon retired to join an expedition to Mexico.

In 1837, he succeeded Loudon as Editor of the *Magazine of Natural History*. He commenced a new series and edited four volumes, terminating his connection in 1840, when the *Magazine* was united with the *Annals of Natural History*, that had been started two years previously.

About the year 1843, Charlesworth planned the publication of the *London Geological Journal*, but the first number did not appear until September, 1846, owing chiefly to the fact that in 1844 he was appointed Curator of the York Museum. The *Journal* was profusely illustrated with plates, and it contained valuable contributions from the leading palæontologists of the day. Charlesworth himself gave an account of the occurrence of flint in the pulp-cavity of a tooth of *Mosasaurus*. Three numbers only of this *Journal* were published, the last being issued in May, 1847.

In 1858, Charlesworth gave up his Curatorship at York, where he was succeeded by the late W. S. Dallas, and eventually settled for a time in London. From this date, he appears to have made a somewhat precarious livelihood, chiefly by the sale of specimens. He formed the "British Natural History Society," which consisted only of himself, and undertook the disposal more especially of the Tertiary

and Recent Mollusca. He prided himself also on being the first to introduce glass-topped boxes for the preservation of delicate specimens. He would appear occasionally at a scientific meeting, when there was a discussion on flints, in which he took an especial interest; and for several years he successively attended the anniversary meetings of the Geological Society, then held at Somerset House, and argued at length, with great fluency, and in loud tones, about the management of the Society's affairs. While his speeches contained, at times, matter well deserving of discussion, his manner too often was aggressive and needlessly offensive; and this want of tact was displayed in some of the critical remarks he introduced into the journals he edited. From these and other causes Charlesworth came gradually to lose position and friends. Starting, as he did, so full of promise, with ability of a high order and much enthusiasm, it is sad to think how in later years he neglected his talents and misused his opportunities, for the work of his early years, though not great, is sufficiently important to form an enduring contribution to the literature of geology.

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## LEONARD BLOMEFIELD (JENYNS).

BORN 1800. DIED SEPTEMBER 1, 1893.

THE last of Darwin's associates in the preparation of "The Zoology of the Voyage of H.M.S. 'Beagle,'" has passed away in the person of the Rev. Leonard Blomefield (formerly Jenyns), who contributed the section on the fishes. He was the youngest son of the late Rev. George Leonard Jenyns, of Bottisham Hall, Cambridgeshire, and for many years had resided at Bath. He graduated at Cambridge, and his first paper on the Ornithology of Cambridgeshire, was read before the Cambridge Philosophical Society in 1825. Between that year and 1833 he also contributed to the Transactions of the same Society papers on the reptiles of Cambridgeshire, the habits of the natterjack toad, a mite parasitic on slugs, a swarm of flies, and on the British species of *Cyclas* and *Pisidium*. In 1834 he presented a report on "The Recent Progress and Present State of Zoology" to the British Association, and in the following year he published at Cambridge his well-known "Manual of British Vertebrate Animals." Eleven years later he also published another small work, entitled "Observations in Natural History: with an Introduction on habits of observing, as connected with the study of that Science. Also a Calendar of Periodic Phenomena in Natural History." He was especially devoted to field observations, and during the last fifty years made many contributions to scientific literature, his last being a Presidential Address to the Bath Natural History and Antiquarian Field Club in 1892. Both in this Club and in the Bath Institution, Mr. Blomefield took the deepest interest, and the Institution is indebted to

him for a fine scientific library of nearly 2,000 volumes, besides a good herbarium.

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## ALEXANDER STRAUCH.

THE death is announced of the eminent herpetologist, who has for many years been Director of the Museum of the Imperial Academy of Sciences at St. Petersburg. Though only in his sixty-second year, Dr. Strauch had been in feeble health for some time before his death, and had been unable to make much progress in his favourite studies. His more important memoirs were published by the Academy, and comprise, among others, an essay on the Herpetology of Algeria (1862), studies of Chelonia (1862, 1865, and 1890), a synopsis of the Crocodilia (1866), a synopsis of the Viperidæ (1869), a revision of the Salamanders (1870), on the Snakes of Russia (1873), and on the Geckos (1887). Dr. Strauch's memoir on the Russian snakes is especially valuable, and it is a misfortune that he was unable to complete similar memoirs on the other reptiles and batrachians of the empire. His contemplated description of the reptiles collected by Przewalski remains unfinished.

# NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

PROFESSOR C. H. TYLER TOWNSEND, of Las Cruces, New Mexico, has been appointed Curator of the Museum and Institute, Kingston, Jamaica.

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MR. L. J. SPENCER, B.A., of Sidney Sussex College, Cambridge, is the successful candidate in the recent examination for the vacant Assistantship in the Mineralogical Department of the British Museum. Mr. Spencer was awarded the Harkness Scholarship last June, and before entering upon his appointment will spend the remainder of this year in visiting the Museums and Universities of the Continent.

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THE Scientific Library of the late Professor J. S. Newberry, has been presented as a memorial to the Geological Department of Columbia College, New York.

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A VALUABLE concise report on the Marine Biological Laboratories of Europe, by Dr. Bashford Dean, appears in the July and August numbers of the *American Naturalist*.

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ACCORDING to *Indian Engineering*, a wealthy citizen of Calcutta has offered a sum of money for the purpose of building a library in the Zoological Gardens of that city. The new laboratory at the Gardens is completed, and will shortly be ready for use.

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THE first of a series of Stratigraphical Memoirs, published by the Geological Survey, was on the Pliocene Deposits of Britain, by Mr. Clement Reid, and was issued in 1890. Now a further instalment has been published, in two volumes, on the Jurassic Rocks of Yorkshire, by Mr. C. Fox-Strangways. These works summarise our knowledge on the various formations and their subdivisions, and they contain full lists of the fossils.

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THE Annual Report of the Madras Museum, 1892–93, records, among other additions, the mounting of a stuffed specimen of the rare shark, *Rhinodon typicus*, cast ashore at Madras in 1889. A figure is given. During the year the Superintendent has visited the corundun deposits, and collected a series of Cretaceous fossils near Pondicherry. A collection of ammonites has been sent to Vienna, for study by Dr. W. Waagen.

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THE Annual Report of the British Museum for 1892 has been issued. The Director of the Natural History Departments states that the building of a temporary room for the accommodation of the Cetacea is under contemplation; and special allusion is made to the inconvenience caused by the want of a lecture room. The Swiney Lectures on Geology, by Professor Nicholson, will be delivered this year in the Lecture Theatre of the neighbouring South Kensington Museum. The subject is the Bearing of Geology on the Geographical Distribution of Plants and Animals, and the course commences on October 2.

A BEAUTIFUL new example of protective resemblance in animals has been added to the collection in the hall of the Natural History Branch of the British Museum. It is a beetle (*Lithinus nigrocristatus*) from Madagascar, living on lichen, and adorned in such a manner as to precisely resemble the latter. A very fine collection of madreporarian corals from Western Australia has just been sent to the Zoological Department by Mr. W. Saville Kent, and a nearly complete skeleton of the extinct New Zealand rail, *Aptornis*, has been added to the exhibited collection in the Geological Department.

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THE political disturbances in Bohemia are seriously retarding the arrangement of the Natural History collections in the new Royal Bohemian Museum at Prague. The annual grant of money this year has been greatly reduced. The cases for the Barrande collection of fossils are still unfinished, and probably will not be ready until next spring; while there is little prospect of the completion of the new cases for the other fossils within the next two years. Considering the lavish expenditure on the architectural features of the Museum, this niggardliness in providing fittings is much to be deplored. For the student of Palæozoic fossils, the Bohemian collection is perhaps the most important in Europe, and any delay in rendering it of service is a great misfortune.

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THE Proceedings of the meeting of the German Anatomical Society, held last May, have just been issued as a supplement to the *Anatomischer Anzeiger*, forming a small volume of 224 pages.

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THE Norfolk and Norwich Naturalists' Society has established a "Yarmouth Section" at Great Yarmouth, with the Rev. C. J. Lucas as Chairman, and Mr. Arthur Patterson as Hon. Sec.

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THE Haslemere Natural History Society is interesting itself in the lectures on Technical Education provided by the Surrey County Council. It has organised a "Teaching Committee" to supplement, by class work, the ordinary lectures.

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THE ninth part of vol. i. of the *Journal of the Trinidad Field Naturalists' Club* has been issued. Besides the reports of meetings, it contains papers on Composite Plants, Mosquitoes, Ticks on an Iguana, etc. Mr. F. W. Urich's account of the mosquito pest is of special interest, and the Club is doing wisely in restricting its operations to local research. Several members are occupied in collecting the mammals of Trinidad, in preparation for a forthcoming work on the subject by Mr. O. Thomas, of the British Museum.

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*Timehri*, the journal of the Royal Agricultural Society of British Guiana, is nearly always the most interesting of Colonial publications. The latest part (vol. vii., pt. i.) is especially readable, and contains some valuable contributions. Mr. James Rodway, the editor, discusses the seasons in Guiana; an account of the Indians of Guiana is translated from an old Dutch work, published in 1770; Mr. H. I. Perkins contributes notes on a journey to the Cuyuni Gold-Mining District; and Mr. H. C. Swan records his experiences of insect-collecting in British Guiana.

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THE newly-issued part of the *Proceedings of the Liverpool Geological Society* (vol. vii., pt. ii.) contains Mr. W. Hewitt's Presidential Address on the New Red Sandstone with reference to its mode of formation, and several other papers of much interest. Mr. Mellard Reade gives an elaborate and well-illustrated account of the Glacial Deposits of North Wales, discussing their possible origin; and Dr. C. Ricketts treats of the conditions under which the older Carboniferous rocks of N.W. England were formed. Messrs. P. Holland and E. Dickson discuss the formation of clay, and there are some descriptive papers on local geology.

THE recent meeting of the British Association at Nottingham (Sept. 13 to 20), though receiving scarcely any communications of absolute novelty, was full of interest; and the arrangements of the local committee were all that could be desired. The attendance, also, though not remarkably large, was at least equal to that of most recent meetings. In addition to the Presidential Addresses, which we notice elsewhere, there were several interesting discussions to attract widespread attention, notably the debates on the nature of life (opened by Dr. J. S. Haldane), on coral reefs (opened by Professor Sollas), and on the place of geology in education (led by Professors Cole and Lebour). A more futile discussion than the attempt to settle the limits of Geology and Geography, we do not remember to have heard.

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THE section for Geology seems to have been favoured with the greatest number of papers, and the Petrologists, as might have been expected, mustered in considerable force. There were several contributions to local geology, Mr. A. T. Metcalfe's description of the gypsum deposits being the most elaborate. Professor Brögger (of Christiania) discussed some eruptive rocks in Norway, and Professor Iddings (of Chicago) described a dissected old volcano in Wyoming. Professor Johnston Lavis presented his usual report on Vesuvius, and Professor Sollas explained the origin of intermediate varieties of igneous rocks by intrusion and admixture. Mr. Walcot Gibson gave a general sketch of the geology of Central East Africa, and Mr. R. D. Oldham exhibited two new geological maps of India. Dr. H. Hicks reiterated his views on the base of the Cambrian Formation; and Mr. Goodchild showed how the "eyes" in gneiss give a clue to its origin. In Glacial Geology, Prince Kropotkin attempted the widest theme in contributing a paper on the glaciation of Asia; Professor Sollas exhibited a map of the eskers of Ireland; Mr. De Rance traced the pre-Glacial form of the ground in Lancashire and Cheshire; and Messrs. Abbott and Kendall had some more remarks on the "heretics" who believe that North Wales was submerged to a great extent in the Glacial Period. Professor Bonney and Mr. Lindvall also spoke on theoretical matters in Glacial Geology. In Palæontology, Mr. E. T. Newton gave the latest information about his Triassic reptiles from Elgin; and Dr. Traquair recorded a cephalaspidian fish from Caithness. In addition to the papers, the usual reports on Underground Waters, Erratic Blocks, Photographs, Palæontology, etc., were presented.

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THE Biological Section had to lament the absence of the President on account of ill-health. Among the more fundamental questions, Professor J. B. Farmer treated of some new features in the division of the nucleus in plant-cells, and Messrs. Hartog and Dixon discussed the digestive ferments of a large protozoon. Messrs. Cattle and Millar read a paper on Gregarines and the possible connection of allied forms with tissue-changes in man, notably with the production of cancer. Mr. W. E. Hoyle described the luminous organs of Cephalopoda, and Mr. F. T. Mott discussed the origin of organic colour. Mr. F. Enock spoke on insect parasites, and Mr. G. B. Rothera on vegetal galls. Mr. W. S. Bruce gave an account of the seals and whales seen during his recent voyage to the Antarctic regions, and Dr. C. H. Hurst made the observations on birds' wings which we print elsewhere. The most generally interesting botanical paper was Miss N. Layard's description of the root of the duckweed (*Lemna*). Mr. J. Clark treated of lime in its relation to some physiological processes in the plant, and Mr. H. Brown had a paper on starch. There were numerous interesting reports of committees, that dealing with the legislative protection of the nests of wild birds leading to the warmest discussion.

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THE Geographers were treated to some entertaining exhibitions of lantern slides, Mr. W. M. Conway bringing his views of the Karakoram Mts., and Professor John Milne showing some pictures of Japan with especial reference to earthquakes. Mr. Murdock also exhibited his paintings lately made in the Antarctic regions. Besides

numerous addresses by travellers, there were papers by Mr. J. Y. Buchanan on the influence of land on the temperature of the air; by Mr. H. N. Dickson on the sea between Scotland and the Faroe Isles; and by Dr. H. R. Mill on the Clyde Sea Area and on the English Lakes.

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The Anthropologists began with Mrs. Grove's paper on the Ethnographic Aspect of Dancing, and had numerous interesting discussions both on Ethnographical and Antiquarian subjects. The event of the meeting seems to have been Mr. A. Bulleid's description of the ancient British village recently explored by him near Glastonbury. Mr. E. W. Brabrook communicated the first report of the committee for undertaking an ethnographic survey of the British Isles; and Professor Hans Hildebrand, of Stockholm, discussed Scandinavian antiquities. Dr. J. H. Gladstone also started a somewhat indecisive discussion on the possibility of recognising a copper age between the ages of stone and bronze.

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The General Committee of the British Association awarded grants of money for investigations in Natural Science as follows:—Erratic blocks, £15; Fossil Phyllopoda, £5; Geological photographs, £10; Shell-bearing deposits, £5; Eurypterids, £5; New sections of Stonesfield Slate, £25; Earth tremors, £50; Exploration of Calf Hole Cave, £5; Table at Naples Zoological Station, £100, and at Plymouth Station, £15; Zoology of Sandwich Islands, £100; Zoology of Irish Sea, £40; Mammalian heart, £10; Climatology and hydrography of Tropical Africa, £10; Observations in South Georgia, £50; Exploration in Arabia, £30; Anthropometric Laboratory statistics, £5; Ethnographical survey of United Kingdom, £10; The Lake Village at Glastonbury, £40; Anthropometrical measurements in schools, £5; Mental and physical condition of children, £20; Corresponding societies, £25. The total amount expended in grants was £705.

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The British Association meets next year at Oxford early in August, under the presidency of the Marquis of Salisbury, Chancellor of the University. A new section for Physiology will then be inaugurated. The invitation of the town of Ipswich has been accepted for 1895.

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The nomination of the Marquis of Salisbury for the Presidency, which has met with general approval, was proposed by Sir Frederick Bramwell and seconded by Sir William Flower. It is rarely that one sees gratitude for past favours so tactfully expressed as in the remarks of Sir William Flower, reported in the *Nottingham Daily Guardian* of September 19. Sir William said: "Lord Salisbury had shown general sympathy with all branches of science for a long time, and he was the first Prime Minister to recognise that men of science might sometimes aspire to honours and distinctions which had hitherto been reserved for successful soldiers or barristers and others. He had made a scientific man on his own merits a member of the peerage, and had appointed another purely scientific man a member of the Privy Council."

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The Toynbee Hall Natural History Society, Whitechapel, is still actively pursuing its good work in the East End of London. During the winter, meetings are regularly held at the Hall, and during the summer cheap excursions, of long and short duration, are organised to various districts of interest. The longest excursion of the past season was a visit of fifteen days to the island of Jersey. The party, numbering seventeen, started on Friday, July 28, and returned Saturday, August 12. The headquarters during the whole of the time were at Gorey, on the S.E. coast of the island. Those taking part in the expedition represented three sections—Botany, Geology, and Zoology. For the purposes of systematic work, a portion of the island was allotted to each day, so that the whole of the coast and much of the interior was covered during the visit.

The effects of the drought in Jersey were not so evident as they are in England, and most of the plants for which the Channel Islands are noted were secured for the Society's herbarium. Many of them were in great abundance, and included—*Mathiola sinuata*, R. Br.; *Polycarpon tetraphyllum*, L.; *Lythrum Hyssopifolia*, L.; *Hypericum linarifolium*, Vahl.; *Gnaphalium luteoalbum*, L.; *Centaurea aspera*, L.; *Armeria plantaginea*, Willd.; *Microcala filiformis*, Link.; *Echium plantagineum*, L.; *Scrophularia Scorodonia*, L.; *Sibthorpia europæa*, L.; *Scilla autumnalis*, L.; *Cyperus longus*, L.; *Scirpus pungens*, Vahl.; *Fibichia umbellata*, Koch.; *Bromus maximus*, Wesf., and *Briza minor*, L.

It is worthy of note that in the north-east part of the island, at least, *Hypericum humifusum*, L., could not be found, but the form *H. decumbens*, Petermann, was abundant on the roadsides. It is, therefore, not very evident that *H. decumbens* is a hybrid between *H. humifusum* and *H. linarifolium*.

The ferns were not good or abundant except in the north-west. Those members inclined for shore work received valuable aid respecting the best hunting grounds, etc., from Mr. Sinel.

The Geologists were able to collect specimens of nearly all the rocks described from the island, including very fine examples of the Pyromerides of Bouley Bay. Owing to the complexity of structure of the island, the relations of the rocks to one another were in many cases not well made out, but the intrusion of the Granite series into the Diorite was well seen on the north. The great series of dykes were examined all along the coast, and the results of weathering, in the formation of hollows and caves, where they cut the harder granites, attracted general attention. A good series of rocks was secured for the Society's museum, and the micro slides, which are in course of preparation, will be exhibited at one of the winter meetings.

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For the last two years, two long excursions, in addition to the usual Saturday and Sunday excursions, have been successfully carried out by Toynbee Hall. The plan of camping has been found to answer well, since the expenses are small; and this is a point of considerable importance, for one great object of the Society is to bring these Natural History outings within the reach of a large number. In the early summer of this year a camp was pitched for eight days at Thursley, Godalming, within a short distance of Hind Head. The cost for eight days' camp, including railway fare, was 24s., and the cost per member for a fortnight in Jersey, including return ticket, was 68s. The Society dispensed with tents in Jersey, as a farm-house was found sufficiently large to accommodate the whole party.

# CORRESPONDENCE.

## Phylogeny and Ontogeny.

I REGRET that Mr. Bather should have troubled himself to reply to me while he had not access to my original paper, and while he was subject to the inevitable distractions and inconveniences of travelling. Under such conditions it was probably impossible for him to avoid inadvertent misrepresentation.

I did not "deny that the past history of its race has *any* influence on the growth-stages of an individual," but only "that the phylogeny can *so* control the ontogeny as to make the latter into a record of the former—even into an imperfect record of it." This, however, is probably what Mr. Bather meant. But there is a very real misrepresentation two or three sentences further down on the same page (238). I have never "maintained that the development of any [=every] individual was a regular progress from the embryo to the adult." Not only am I pretty familiar with Weismann's work on the Diptera, but I have myself worked out in some detail the development of *Culex*, in which the alimentary, respiratory, muscular, and nervous systems and, to a smaller extent, other systems and organs also, exhibit phenomena (histolysis, for instance) which could not by any stretch of the imagination be called "regular progress from embryo to adult." The phenomenon called "metamorphosis" is too familiar for any zoologist to make any such statement as that ascribed to me.

I have already answered Mr. Bather's question elsewhere, but as he says "it is needless to reply" to my "other remarks while the above question remains unanswered," I will answer it again; merely premising that he has so far not replied to any of *my* "remarks" on the subject under consideration, but only to "remarks" which he has mistakenly ascribed to me.

His question is this:—"What cause can have produced these deviations of ontogeny from the path of simple development?" the deviations in question being those which he described in NATURAL SCIENCE, vol. ii., pp. 275 *et seqq*. My answer is that the species have so varied under the guidance of Natural Selection that the *later* stages of development have come to differ more widely from the *corresponding* stages of the ancestors than the *early* stages have come to differ from the *corresponding* stages of the same ancestors. The *vestiges*—I fully admit that they *are* vestiges—still to be found in the embryos of the existing species in the case of *Antedon*, are surviving remnants of the rudiments which existed in the ancestral embryos. They are so modified that, though still recognisable, they do not now any longer follow the same course of development as they did in those ancestors. Mr. Bather will remember that this explanation was given with reference to the "gill-arches" of embryonic birds and mammals (vol. ii., p. 198).

As the *Philosophical Transactions* and the *Annals and Magazine of Natural History* are practically inaccessible to a large proportion of the readers of NATURAL SCIENCE, I will again express the hope that Mr. Bather will take an early opportunity of putting the more interesting and tangible results obtained by Dr. Carpenter and himself before those readers, with such figures as may put them beyond the possibility of being misunderstood. (*Cf.* Mr. Smith Woodward's article on "The Fore-runners of the Back-boned Animals," vol. i., p. 596). I feel sure that by so doing he will facilitate the solution of our present problem.

C. HERBERT HURST.

Manchester, Sept. 10, 1893.

[We shall welcome any such concise statement of facts as is suggested by Dr. Hurst; otherwise this correspondence must now cease.—ED.]

## The Moas of New Zealand.

Natural Science has honoured me with two reviews of my paper on "The Moas of New Zealand," one in October, 1892, by Mr. R. Lydekker, the other in May, 1893, by Mr. H. O. Forbes, and by these I am gratified. I hope, however, I may be allowed a little space for a few additional remarks, because, in the first review, Mr. Lydekker has, unintentionally, I am sure, misrepresented my opinion on an important point; while in the second review there are two misstatements of facts which I ought not to leave unnoticed.

Mr. Lydekker in his criticism (Nat. Sci., vol. i., p. 594) says that as I admit that the Moas probably passed from New Zealand to Australia during the Pliocene (? Pleistocene) period without the passage of any mammalia in the opposite direction, my arguments as to the impossibility of flightless birds, as such, having reached Australia and New Zealand at an earlier period, are by no means convincing. But in my paper I say that the migration from New Zealand to Australia could not have been later than the Eocene period (*Trans. N. Z. Inst.*, vol. xxiv., p. 147); and I point out how, by an isolation of a part of the New Zealand area and its subsequent connection with Australia, this latter country could have been inoculated with Struthious birds without any possibility of mammalia passing in the opposite direction. But it would be highly improbable to suppose that on a part of Australia the Struthious birds had been isolated from the mammalia and then transferred alone to New Zealand. The two cases are quite different. No doubt a little further on in my paper I say that *if Mr. De Vis is right* in referring a femur from Darling Downs to Dinornis, *then* we should have to allow a second migration from New Zealand to Australia in the Pliocene; and this may have misled Mr. Lydekker. But it is, I think, evident that I was very doubtful about this, and now, having examined a cast of the Queensland bone, I find that it differs considerably from any genus belonging to the Dinornithidæ, and resembles more closely the femur of the Emu and Cassowary, especially that of the young Emu,[1] so that there is no longer any reason for supposing a late migration of Struthious birds between New Zealand and Australia.

Turning now to the second review. In my paper I have a footnote regretting that I had not been able to see Mr. Lydekker's "Catalogue of the Fossil Birds in the British Museum," of which "no copy has as yet been received in Christchurch" (*l.c.*, p. 98). Nevertheless Mr. Forbes says, "It is greatly to be regretted by all workers on this most difficult subject that Mr. Hutton did not defer the publication of this valuable paper, in which has been brought together almost all the known information on the Moa, till he had found time to compare his nomenclature with that of Mr. Lydekker's catalogue—a volume which had already reached the Colony before the reading of his paper" (Nat. Sci, vol. ii., p. 377).

Now the first part of my paper "On the Classification of the Moas" was read on October 1, 1891, and the second part, "On the History of the Moa," on November 4, 1891 (both dates appearing on the face of the published paper); while in the books of the Christchurch Museum I find an entry, *in Mr. Forbes's own handwriting*, that Lydekker's "Catalogue of the Fossil Birds in the British Museum" was received on November 18, 1891; and as this is the first copy that reached Christchurch, it is evident that both parts of my paper had been read, and had left my hands some time before the book was received here. As a matter of fact, I was not aware, when I read the first part of my paper, that Mr. Lydekker had been working at the same subject. I saw a notice of his book a few days afterwards, and went to the Museum to ask Mr. Forbes, who was then curator, if he had got it; and on his replying in the negative, I added the footnote to my paper. I mention this because I do not wish Mr. Lydekker to think that I treated in so cavalier a fashion his very valuable work, from which I have learnt a great deal. I could, of course, have written to Wellington and asked leave to withdraw my paper, but it did not seem to me worth while to do this, because none of the new species described by me are identical with those described by Mr. Lydekker, and the only changes I could

[1] On *Dinornis* (?) *Queenslandiæ*, by Captain F. W. Hutton, F.R.S., in *Proc. Linn. Soc. N. S. Wales* [2], vol viii. (1893), pp. 7–11.

have made were a few in the genera, owing to Mr. Lydekker having convinced me that I had put too much reliance on Sir R. Owen's plates.

The second point in Mr. Forbes's criticism to which I wish to refer relates to the age of th ebeds in which *Anomalopteryx antiqua* was found. Mr. Forbes says (Nat. Sci., vol. ii., p. 378), "I shall, however, most willingly admit that I am mistaken as to the age of the strata in which these *Dinornis* remains have been found as being other than "newer Pliocene, or even Pleistocene," so soon as the officers of the New Zealand Geological Survey—who are really the only competent referees in the case—shall have assigned to these gravels a different age." But it was the officers of the Geological Survey whom Mr. Forbes contradicted. It was Sir Julius von Haast, Provincial Geologist of Canterbury, who referred the Timaru lava stream, which overlies the gravels, to the Eocene; and it was Mr. A. McKay, Assistant Geologist to the Geological Survey of New Zealand, who said that these beds are of Miocene age. All that I did in the matter was to point out that Mr. Forbes differed from the officers of the Geological Survey without examining the evidence on which their opinions were founded.

Canterbury Museum, Christchurch, N.Z. F. W. Hutton.
25 June, 1893.

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I have to thank the Editor for the perusal of the above observations of Mr. Hutton. In regard to the date of the arrival in New Zealand of Mr. Lydekker's "Catalogue of Fossil Birds in the British Museum," I beg to say that the date entered in the Museum Additions Book does not *necessarily* indicate the exact date of its receipt. As Mr. Hutton says that he came to me for the volume after the reading of the *first* part of his paper, I leave the question there. I handed him Mr. Lydekker's volume, however, immediately on its reaching my hands, *within a day or two* of his asking me for it. At all events, the point I raised remains unaltered, that as Mr. Hutton's paper could not be published for at least five or six months after it was read, and since Lydekker's classification and the descriptions of his new species were in Mr. Hutton's hands before he corrected his proofs, he ought not to have added to the confusion already existing in the classification and synonymy of the Dinornithidæ.

In regard to the second point, I quoted in my paper the latest opinion of the Director of the Geological Survey, which I have now before me, and if I recollect aright the opinion verbally expressed to me by Mr. A. McKay on the last occasion on which he visited me at the Museum.

Henry O. Forbes.

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### On *Anomalopteryx antiqua*, Hutton, and other New Species of Moa from Enfield, New Zealand.

In the *Transactions of the New Zealand Institute*, vol. xxv. (1892), just received, Mr. Hutton gives a further account of this species founded by him in the previous volume on fragments of a tibia embedded in two blocks of laterite found near Timaru, N.Z., and on two other fragments of a metatarsus he had seen in a photograph, which he assumes to belong to the same species. The photograph is reproduced on plate xvii. of volume xxiv. The species is founded on the tibial fragments marked *a* and *b*; and on those of the metatarsus lettered *c* and *d*. In the same volume, page 125, he states the length of the latter to be "5·5 ? inches," and of the former to be "12 inches." In the new volume he now, on plate iv., figures the two metatarsal fragments from their casts made by me, which he had previously overlooked in the Christchurch Museum. The interest of the specimens rests in their being the oldest—Newer Pliocene or Pleistocene—known portions of *Dinornis*; but the value of *Anomalopteryx antiqua* as a species, and necessarily the value of the deductions drawn from these fragments of bone, will be apparent when I state that fragment *b* of the tibia was obtained at one time (the first find) and the fragment *a* on a subsequent occasion (the third find), and that they came from different "drives" made for blasting purposes, under distant parts of the dolerite

sheet. In like manner, the fragments *c* and *d* also were found, as I personally know, in different drives far apart from each other. The chances, therefore, of these fragments all belonging to the same bird are infinitely little.

Each may represent a different species, or even a different genus. Mr. Hutton has, however, boldly deduced the length of the *tibia* of his new species from the one set of fragments, and its metatarsus from the other set. On this compound structure he has built conclusions as to the early forms of *Dinornis*, and as to the pedigree of others. The author observes, on p. 16, "It will be seen that my inference that the Moa bones from Timaru belonged probably to the genus *Anomalopteryx* was a correct one."

If we now turn to his own diagnosis of the genus (tom. cit., xxiv., p. 123) we find it to be based on certain characters of the skull, sternum, and pelvis—bones which, in the present species, are all unknown; and on the metatarsus being shorter than the femur (here also undiscovered), and on its length being 2·0–2·3 times the girth of the shaft, which again is lost in both fragments. *Anomalopteryx* is further defined by its tibia being "2·1 to 2·2 times the length of the metatarsus, and its breadth between 4·2–5·1 times the girth." As already pointed out, the tibial fragments are "much hidden in the matrix," and imperfect at both ends, and Mr. Hutton now admits that "the length of the metatarsus is unknown." There is left, therefore, not a single character by which these fragments can be referred to *Anomalopteryx*, as defined by him, or to any other genus or species.

Of the additional species described in this volume as new, nearly all are based on bones exhumed at Enfield, and examined with considerable care by myself. When arranged in a series of several yards in length, the one bone merged into its neighbour so gradually that it was quite impossible to draw any line and say the bones to the right ought to belong to one species, those on the left to another. The bones associated together by Mr. Hutton to form a limb are purely guess-work, and may or may not belong to one species (not to say individual) and the comparative dimensions of its different parts can have only conjectural value. Indeed, most of the separate bones might easily be fitted to different existing species, for it is well-known that the bones of birds of ancient pedigree vary greatly in their dimensions. From descriptions such as are given by Mr. Hutton, unaccompanied by figures, it is impossible to identify the species of *Dinornis* he has established. They merely increase the already almost inextricable confusion which those who are working on this subject are compelled laboriously and unnecessarily to unravel.

HENRY O. FORBES.

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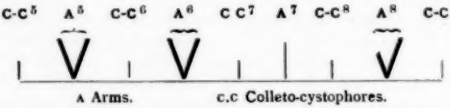
### LATENT CONGENITAL VARIATION IN A LUCERNARIAN?

IT seems to be the fact that the Lucernarians examined by Mr. Hornell, and described in his note in the July number, were abnormal when gathered. On January 6 of this year I received from Messrs. Sinel & Hornell two specimens of *Haliclystus octoradiatus*, normal as to the number of arms, c-cystophores, and septa. They were put into a tank in good order, and containing a fair supply of food in the shape of small crustacea. On the 8th, no abnormality was perceived, and illness prevented my visiting the tank for some days; but when next seen, one specimen had eleven tentacular arms instead of eight. The other specimen continued normal till death.

The per-radial joining c-c$^{1}$ and c-c$^{8}$—which were slightly smaller than the others—divided the animal into an upper abnormal and a lower normal half. The latter calls for no observation. Between c-c$^{8}$ and c-c$^{6}$ were two arms well developed; between c-c$^{6}$ and c-c$^{7}$ were also two arms, but that next c-c$^{7}$ was smaller than its neighbour; there was a single arm between c-c$^{7}$ and c-c$^{8}$; and between c-c$^{8}$ and c-c$^{1}$ arm$^{8}$ was duplicated.

As I felt unable to deal with the problem, I offered the animal to Professor E. Ray Lankester, at whose wish it was sent to Mr. Garstang, at Plymouth, who, with regard to the last-mentioned arm, wrote to me thus:—"Arm$^{8}$ has now only one bunch of tentacles; but there is an abrasion on one of its edges, near c-c$^{8}$, and possibly one of the supernumerary arms has sloughed away from that spot."

Examination showed him that the septa in the upper half were four in number instead of two. The following diagram represents the abnormal half as it was when it reached Plymouth :—




Mr. Garstang was good enough to preserve the specimen, and it is now at Plymouth, so as to be available for anyone working there.

My first impression was that the supernumerary arms arose by fission of the normal ones (though it is difficult to account for the non-fission of arm 7). This was also Mr. Garstang's opinion; and though my rough notes state that the supernumerary arms were, when first observed, smaller than the others, and arose as outgrowths, the notes were made under such disadvantages that I willingly withdraw them in favour of his view.

Henry Scherren.

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## ERRATUM.

Page 106. The lines under the heading "Abyssinian Sub-Region" should be transferred to the heading "South African Sub-Region" below.